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p. 246–7 Professor Dionne Raymond's.

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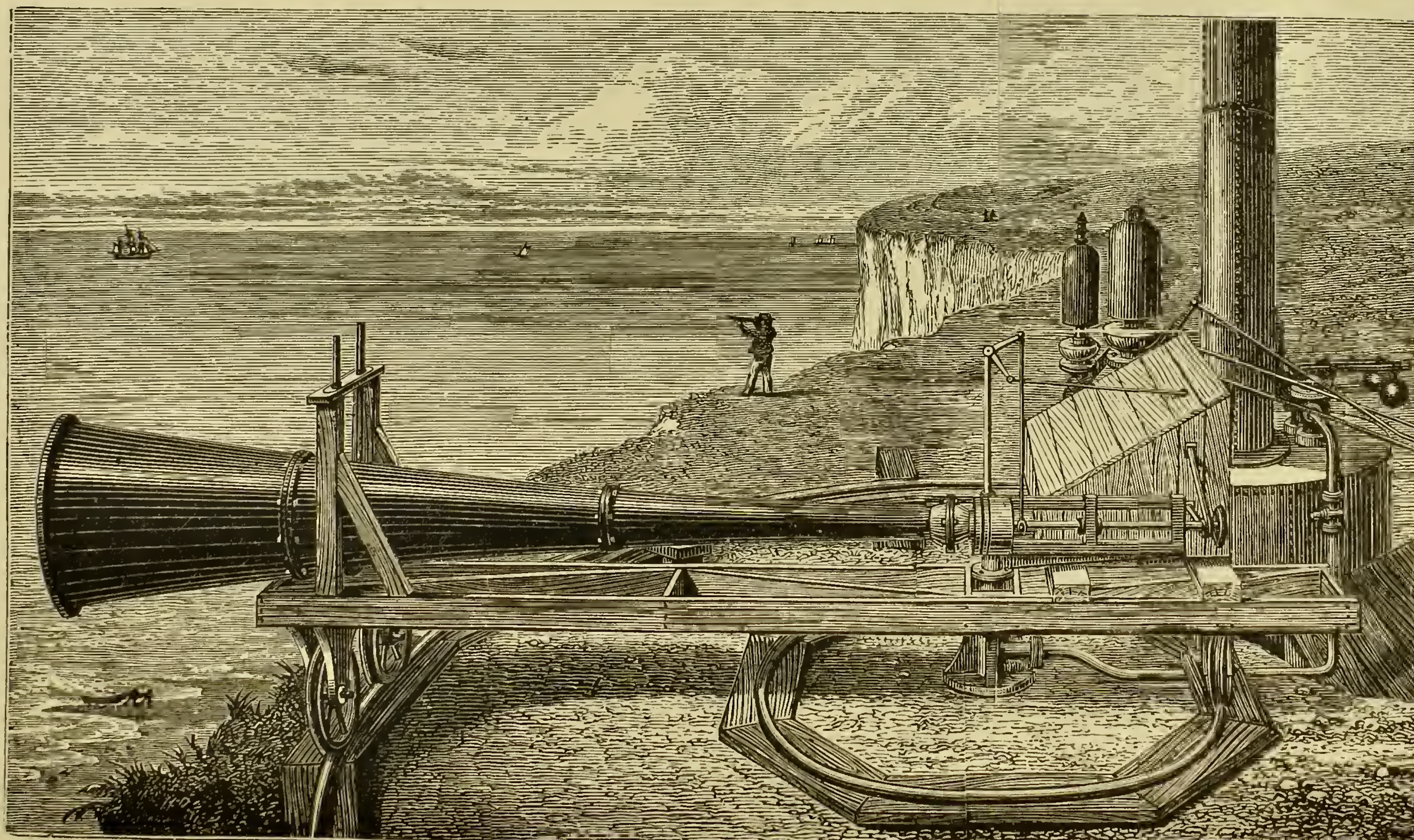

FOG-SYREN

SOUND

BY

JOHN TYNDALL, D.C.L., LL.D., F.R.S.

LATE H[illegible] PHILOSOPHY IN THE
[illegible]

FIFTH EDITION (APRIL 1893)

EIGHTH IMPRESSION

LONGMANS, GREEN, AND CO.
[illegible] PATERNOSTER ROW, LONDON
AND BOMBAY
1902

Frontispiece.

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PREFACE

TO

THE FIFTH EDITION.

THE extracts here given from the prefaces to former editions will enable the reader to note what may be called the historic development of the present work. To each succeeding edition I have added an account of the more recent work done by myself and others. As stated elsewhere, the work has appeared in various languages, and, while subjected to justifiable criticism, it has been, on the whole, exceedingly well received. A reviewer of a French translation wrote complainingly of the absence of mathematics from the work. On the other hand, Helmholtz and Wiedemann, who supervised the German translation, wrote approvingly of the manner in which even the more difficult problems of acoustics had been dealt with by a purely experimental method. The learned Germans had seized my object more correctly than the French reviewer. To introduce mathematics into the volume would, from my point of view, have been to ruin it. Like my work on Heat, these lectures on Sound were intended to rouse the public mind to a sense of the interest and importance and, if possible, to the fascinations of physical science. My aim throughout has been to stimulate as much as to instruct.

HIND HEAD: *April*, 1893.

Extract from the Preface to the First Edition.

In the following pages I have tried to render the science of Acoustics interesting to all intelligent persons, including those who do not possess any special scientific culture.

The subject is treated experimentally throughout, and I have endeavoured so to place each experiment before the reader that he should realise it as an actual operation. My desire, indeed, has been to give distinct images of the various phenomena of acoustics, and to cause them to be seen mentally in their true relations.

I have been indebted to the kindness of some of my English friends for a more or less complete examination of the proof-sheets of this work. To my celebrated German friend Clausius, who has given himself the trouble of reading the proofs from beginning to end, my special thanks are due and tendered.

There is a growing desire for scientific culture throughout the civilised world. The feeling is natural, and, under the circumstances, inevitable. For a power which influences so mightily the intellectual and material action of the age could not fail to arrest attention and challenge examination. In our schools and universities a movement in favour of science has begun which, no doubt, will end in the recognition of its claims, both as a source of knowledge and a means of discipline. If by showing, however inadequately, the methods and results of physical science to men of influence who derive their culture from another source, this book should indirectly aid in promoting the movement referred to, it will not have been written in vain.

1867.

Extract from the Preface to the Third Edition.

In preparing this new edition of 'Sound,' I have carefully gone over the last one, amended as far as possible its defects of style and matter, and paid at the same time respectful attention to the criticisms and suggestions which the former editions called forth.

The cases are few in which I have been content to reproduce what I have *read* of the works of acousticians. I have sought to make myself experimentally familiar with the ground occupied, trying, in all cases, to present the illustrations in the form and connection most suitable for educational purposes.

Though bearing its due share of the imperfection which cleaves to all human effort, the work has already found its way into the literature of various nations of diverse intellectual standing. Last year a new German edition was published under the special supervision of Helmholtz and Wiedemann. That men so eminent, and so overladen with official duties, should add to these the labour of examining and correcting every proof-sheet of a work like this, shows that they consider it to be what it was meant to be—a serious attempt to improve the public knowledge of science. It is specially gratifying to me to be thus assured that not in England alone has the book met a public want, but also in that learned land to which I owe my scientific education.

June, 1875.

Preface to the Fourth Edition

VARIOUS additions and corrections have been made to this edition of 'Sound.'

Last year I presented to the Royal Society a lengthy memoir on the 'Action of Free Molecules upon Radiant Heat, and its Conversion thereby into Sound.' The methods and results of this investigation are here set forth.

Last year, likewise, I communicated to the Royal Society a short paper on the 'Soundless Zones' observed by General Duane to surround the fog-signal stations on the coast of Maine. The explanation given in that paper of this most remarkable phenomenon is reproduced and illustrated in the present work. It will, I think, stand criticism.

Brief descriptions of the telephones of Bell and Edison, of the microphone, and of the phonograph, are introduced.

Experiments are described touching the refraction of sound at the limiting surfaces of different gases. The effect of a change of velocity is thus rendered strikingly manifest, while the student is helped to a clear conception of the consequences of a similar change in regard to light.

The observations on fog-signalling described in Lecture VII. have been somewhat amplified, and rendered more intelligible by the introduction of maps.

The stoppage of sound by transparent but non-homogeneous atmospheres, and its comparatively free transmission through dense fumes, through showers of bran, paper-scraps, and water, through textures woollen and otherwise, and through felt—in short, through all substances sufficiently porous to preserve the continuity of the air within them—are further illustrated.

March, 1883.

CONTENTS.

LECTURE I.

LECTURE II.

LECTURE III.

LECTURE IV.

LECTURE V.

LECTURE VI.

LECTURE VII.

RESEARCHES ON THE ACOUSTIC TRANSPARENCY OF THE ATMOSPHERE IN RELATION TO THE QUESTION OF FOG-SIGNALLING.

PART I.

PART II.

Investigation of the Causes which have hitherto been supposed effective in preventing the transmission of Sound through the Atmosphere.

LECTURE VIII.

LECTURE IX.

APPENDIX I.

APPENDIX II.

ILLUSTRATIONS.

SOUND.

LECTURE I.

PRODUCTION AND PROPAGATION OF SONOROUS MOTION—SOUNDING BODIES IN VACUO—DEADENING OF SOUND BY HYDROGEN—PROPAGATION OF SOUND THROUGH AIR OF VARYING DENSITY—REFLECTION OF SOUND—ECHOES—REFRACTION OF SOUND—DIFFRACTION OF SOUND—INFLUENCE OF TEMPERATURE ON VELOCITY—INFLUENCE OF DENSITY AND ELASTICITY—NEWTON'S CALCULATION OF VELOCITY—LAPLACE'S CORRECTION OF NEWTON'S FORMULA—RATIO OF SPECIFIC HEATS AT CONSTANT PRESSURE AND AT CONSTANT VOLUME DEDUCED FROM VELOCITIES OF SOUND—MECHANICAL EQUIVALENT OF HEAT DEDUCED FROM THIS RATIO—INFERENCE THAT ATMOSPHERIC AIR POSSESSES NO SENSIBLE POWER TO RADIATE HEAT—VELOCITY OF SOUND IN DIFFERENT GASES—VELOCITY IN LIQUIDS AND SOLIDS—INFLUENCE OF MOLECULAR STRUCTURE ON THE VELOCITY OF SOUND.

§ 1. *Character of Sonorous Motion. Experimental Illustrations.*

18 May

THE various nerves of the human body have their origin in the brain, which is the seat of sensation. When a finger is wounded, the sensor nerves convey to the brain intelligence of the injury; while if these nerves be severed, however serious the hurt may be, no pain is experienced. We have the strongest reason for believing that what the nerves convey to the brain is in all cases *motion*. The motion here meant is not, however, that of the nerve as a whole, but of its molecules or smallest particles.[1]

[1] The rapidity with which an impression is transmitted through the nerves of the frog, as first determined by Helmholtz and confirmed by Du Bois Raymond, is 93 feet a second. More recent measurements make it probable that in man and the higher animals the rate of propagation is much more rapid.

In the different parts of the brain to which they are communicated, the molecular motions of the nerves excite sensations of different kinds. Thus the motions sent forward from the tongue and palate have their correlative in the sense of taste; the motions sent from the retina along the optic nerve awake the sense or consciousness of light; while the motions with which we are now more especially concerned, and which are transmitted by the *auditory nerve*, produce in the brain the sensation of sound.

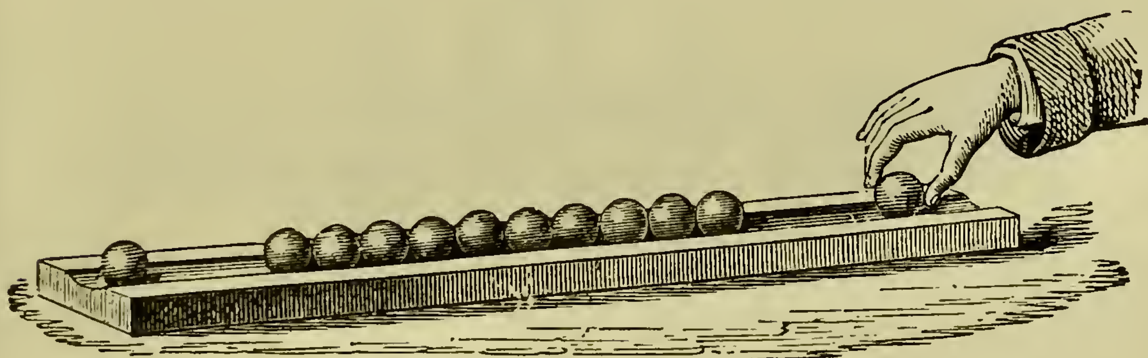
Applying a flame to a small collodion balloon which contains a mixture of oxygen and hydrogen, the gases explode, and every ear in this room is conscious of a shock, which we name a sound. How was this shock transmitted from the balloon to our organs of hearing? Have the exploding gases shot the air-particles against the auditory nerve as a gun shoots a ball against a target? No doubt, in the neighbourhood of the balloon, there is to some extent a propulsion of particles; but no particle of air from the vicinity of the balloon reached the ear of any person here present. The process was this: When the flame touched the mixed gases they combined chemically, and their union was accompanied by the development of intense heat. The heated air expanded suddenly, forcing the surrounding air violently away on all sides. This motion of the air close to the balloon was rapidly imparted to that a little further off, the air first set in motion coming at the same time to rest. The air, at a little distance, passed its motion on to the air at a greater distance, and came also in its turn to rest. Thus each shell of air, if I may use the term, surrounding the balloon took up the motion of the shell next preceding, and transmitted it to the next succeeding shell, the motion being thus propagated as a *pulse* or *wave* through the air.

The motion of the pulse must not be confounded with the motion of the particles which at any moment constitute

the pulse. For while the wave moves forward through considerable distances, each particular particle of air makes only a small excursion to and fro.

The process may be rudely represented by the propagation of motion through a row of glass balls, such as are employed in the game of *solitaire*. Placing the balls along a groove thus, fig. 1, each of them touching its neighbour, and urging one of them against the end of the row; the motion thus imparted to the first ball is delivered up to the second, the motion of the second is delivered up to the third, the motion of the third is imparted to the fourth; each ball, after having given up its motion, re-

Fig. 1.



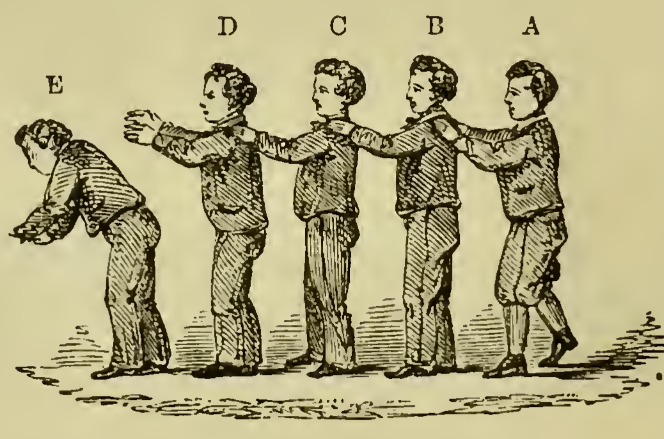
turning itself to rest. The last ball only of the row flies away. In a similar way is sound conveyed from layer to layer through the air. The air which fills the external cavity of the ear is finally driven against *the tympanic membrane*, which is stretched across the passage leading from the external air towards the brain. The vibrations of this membrane, which closes outwardly the ‘drum’ of the ear, are transmitted through a series of bones to another membrane, which closes the drum inwardly, thence through water to the ends of the auditory nerve, and afterwards along that nerve to the brain. Here the physical becomes psychical, mechanical vibrations giving birth to the consciousness of sound.

The possession of clear fundamental ideas is so impor-

* The membrane which is struck (TYMPANUM

tant, that I propose to illustrate the propagation of sound by another homely but useful experiment. I have here five young assistants, A, B, C, D, and E, fig. 2, placed in a row, one behind the other, each boy's hands resting against the back of the boy in front of him. E is now foremost, and A finishes the row behind. I suddenly push A, A pushes B, and regains his upright position; B pushes C; C pushes D; D pushes E; each boy, after the transmission of the push, becoming himself erect. E, having nobody in front, is thrown forward. Had he been standing on the edge of a precipice, he would have fallen over; had he

FIG. 2.



stood in contact with a window, he would have broken the glass; had he been close to a drum-head, he would have shaken the drum. We could thus transmit a push through a row of a hundred boys, each particular boy, however, only swaying to and fro. Thus, also, we send sound through the air, and shake the drum of a distant ear, while each particular particle of the air concerned in the transmission of the pulse makes only a small oscillation.

But we have not yet extracted from our row of boys all that they can teach us. When A is pushed he may yield languidly, and thus tardily deliver up the motion to his neighbour B. B may do the same to C, C to D, and D to E. In this way the motion might be transmitted with comparative slowness along the line. But A, when pushed,

may, by a sharp muscular effort and sudden recoil, deliver up promptly his motion to B, and come himself to rest; B may do the same to C, C to D, and D to E, the motion being thus transmitted rapidly along the line. Now, this sharp muscular effort and sudden recoil is analogous to the *elasticity* of the air in the case of sound. In a wave of sound, a lamina of air, when urged against its neighbour lamina, delivers up its motion and recoils; and the more rapid this delivery and recoil, or in other words the greater the elasticity of the air, the greater is the velocity of the sound.

A very instructive mode of illustrating the transmission of a sound-pulse is furnished by the apparatus represented

Fig. 3.

in fig. 3, devised by my assistant, Mr. Cottrell. It consists of a series of wooden balls separated from each other by spiral springs. On striking the knob A, a rod attached to it impinges upon the first ball B, which transmits its motion to C, thence it passes to E, and so on through the entire series. The arrival at D is announced by the shock of the terminal ball against the wood, or, if we wish, by the ringing of a bell. Here the elasticity of the air is represented by that of the springs. The pulse may be rendered slow enough to be followed by the eye.

Scientific education ought to teach us to see the invisible as well as the visible in nature; to picture with the vision of the mind those operations which entirely elude bodily vision. With regard to the point now under consideration, we must endeavour to form a definite image of a wave of sound. We must be able to see mentally the air particles when urged outwards by the explosion of our

balloon crowding closely together; the particles immediately behind this condensation being separated more widely apart. We must, in short, be able to seize the conception that a sonorous wave consists of two portions, in the one of which the air is more dense, and in the other of which it is less dense than usual. A condensation and a rarefaction, then, are the two constituents of a wave of sound.

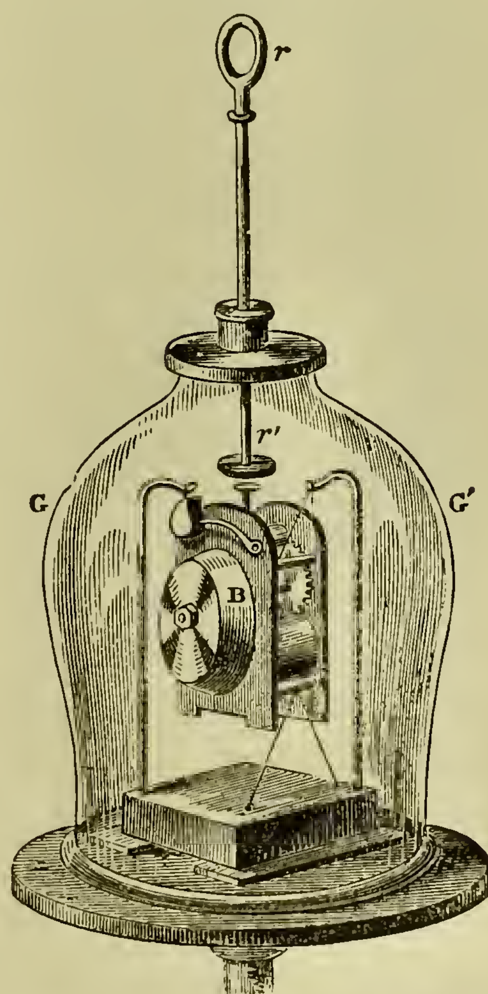
§ 2. *Experiments in Vacuo, in Hydrogen, and on Mountains.*

That air is thus necessary to the propagation of sound was first experimentally proved by the illustrious Robert Boyle. His experiment, after an interval of apparent oblivion, was revived by Hawksbee, in 1705. A bell was so fixed within the receiver of an air-pump that the bell could be rung when the receiver was exhausted. Before the air was withdrawn the sound of the bell was heard within the receiver; after the air was withdrawn the sound became so faint as to be hardly perceptible. An arrangement is before you which enables us to repeat this experiment in a very perfect manner. Within this jar, G G′, fig. 4, resting on the plate of an air-pump, is a bell, B, associated with clockwork.[1] After the jar has been exhausted as perfectly as possible, I release, by means of a rod, *r r′*, which passes air-tight through the top of the vessel, the detent which holds the hammer. It strikes, and you see it striking, but only those close to the bell can hear the sound. Hydrogen gas, which you know is fourteen times lighter than air, is now allowed to enter the vessel. The sound of the bell is not augmented by the

[1] A very effective instrument, presented to the Royal Institution by Mr. Warren De La Rue.

presence of this attenuated gas, though the receiver is now full of it. By working the pump, the atmosphere round the bell is rendered still more attenuated. In this way we obtain a vacuum more perfect than that of Boyle or Hawksbee, and this is important, for it is the last traces of air that are chiefly effective in this experiment. You now see the hammer striking the bell, but you hear no sound. Even when the ear is placed against the exhausted receiver, not the faintest tinkle is heard. The bell is suspended by strings, for if it were allowed to rest upon the plate of the air-pump the vibrations would be communicated to the plate, and thence transmitted to the air outside.

FIG. 4.



Permitting the air to re-enter the jar with as little noise as possible, you immediately hear a feeble sound, which grows louder as the air becomes more dense, until finally every person in this large assembly distinctly hears the ringing of the bell.[1]

Sir John Leslie found hydrogen singularly incompetent to act as the vehicle of the sound of a bell rung in the gas. ~~More than this~~, he emptied a receiver like that before you of half its air, and plainly heard the ringing of

[1] By directing the beam of an electric lamp on glass bulbs filled with a mixture of equal volumes of chlorine and hydrogen, I have caused the bulbs to explode in vacuo and in air. The difference, though not so striking as I at first expected, was perfectly distinct.

The meaning is not quite clear. He found hydrogen to be, as regards transmission, worse than nothing

the bell. On permitting hydrogen to enter the half-filled receiver until it was wholly filled, the sound sank until it was scarcely audible. This result remained an enigma until it received a simple and satisfactory explanation at the hands of Professor Stokes. When a common pendulum oscillates, it tends to form a condensation in front and a rarefaction behind. But it is only *a tendency*; the motion is so slow that the highly elastic air moves away in front before it is sensibly condensed, and fills the space behind before it can become sensibly dilated. Hence sonorous waves or pulses are not generated by the pendulum. It requires a certain sharpness of shock to produce the condensation and rarefaction which constitute a wave of sound in air.

The more elastic and mobile the gas, the more able will it be to move away in front and to fill the space behind, and thus to oppose the formation of rarefactions and condensations by a vibrating body. Now, hydrogen is much more mobile than air; and hence the production of sonorous waves in it is attended with greater difficulty than in air. A rate of vibration quite competent to produce sound-waves in the one may be wholly incompetent to produce them in the other. Both calculation and observation prove the correctness of this explanation, to which we shall again refer.

At great elevations in the atmosphere sound is sensibly diminished in loudness. De Saussure thought the explosion of a pistol at the summit of Mont Blanc to be about equal to that of a common cracker below. I have several times repeated this experiment; first, in default of anything better, with a little tin cannon, the torn remnants of which are now before you, and afterwards with pistols. What struck me most was the absence of that density and sharpness in the sound which characterise it at lower elevations. The pistol-shot resembled the explosion of a

champagne bottle, but it was still loud. The withdrawal of half an atmosphere does not very materially affect our ringing bell, and air of the density found at the top of Mont Blanc is still capable of powerfully affecting the auditory nerve. That highly attenuated air is able to convey sound of great intensity is forcibly illustrated by the explosion of meteorites at elevations where the tenuity of the atmosphere must be extreme. Here, however, the initial disturbance must be exceedingly great.

The motion of sound, like all other motion, is enfeebled by its transference from a light body to a heavy one. When the receiver which has hitherto covered our bell is removed, you hear how much more loudly it rings in the open air. When the bell was covered the aerial vibrations were first communicated to the heavy glass jar, and afterwards by the jar to the air outside; a great diminution of intensity being the consequence. The action of hydrogen gas upon the voice is an illustration of the same kind. The voice is formed by urging air from the lungs through an organ called the larynx, where it is thrown into vibration by the *vocal chords*, sonorous waves being thus generated. But when the lungs are filled with hydrogen, the vocal chords on speaking produce a vibratory motion in the hydrogen, which then transfers the motion to the outer air. By this transference from a light gas to a heavy one the voice is so weakened as to become a mere squeak.[1]

The intensity of a sound depends on the density of the air in which the sound is generated, and not on that of the air in which it is heard.[2] Supposing the summit of Mont Blanc to be equally distant from the top of the Aiguille

[1] In this experiment well-purified hydrogen is placed in an india-rubber bag; the lungs are then emptied and the gas inhaled. The effect is so curious that the speaker can hardly rid himself of the idea that he is imposing upon the hearer.

[2] *Poisson Mécanique*, vol. ii. p. 707.

Verte and the bridge at Chamouni; and supposing two observers stationed, the one upon the bridge and the other upon the Aiguille: the report of a cannon fired on Mont Blanc would reach both observers with the same intensity, though in the one case the sound would pursue its way through the rare air above, while in the other it would descend through the denser air below. Again, let a straight line equal to that from the bridge at Chamouni to the summit of Mont Blanc be measured along the earth's surface in the valley of Chamouni, and let two observers be stationed, the one on the summit and the other at the end of the line; the report of a cannon fired on the bridge would reach both observers with the same intensity, though in the one case the sound would be propagated through the dense air of the valley, and in the other case would ascend through the rarer air of the mountain. Finally, charge two cannon equally, and fire one of them at Chamouni and the other at the top of Mont Blanc; the one fired in the heavy air below may be heard above, while the one fired in the light air is unheard below.

§ 3. *Intensity of Sound. Law of Inverse Squares.*

In the case of our exploding balloon the wave of sound expands on all sides, the motion produced by the explosion being thus diffused over a continually augmenting mass of air. It is perfectly manifest that this cannot occur without an enfeeblement of the ~~motion~~. Take the case of a thin shell of air with a radius of one foot, reckoned from the centre of explosion. A shell of the same thickness, but of two feet radius, will contain four times the quantity of matter; if its radius be three feet, it will contain nine times the quantity of matter; if four feet, it will contain sixteen times the quantity of matter,

and so on. Thus the quantity of matter set in motion *augments* as the square of the distance from the centre of explosion. The intensity or loudness of sound *diminishes* in the same proportion. We express this law by saying that the intensity of the sound *varies inversely as the square of the distance.*

Let us look at the matter in another light. The mechanical effect of a ball striking a target depends on two things—the weight of the ball, and the velocity with which it moves. The effect is proportional to the weight simply; and it is proportional to the square of the velocity. The proof of this is easy, but it belongs to ordinary mechanics rather than to our present subject. Now, what is true of the cannon-ball striking a target is also true of air striking the tympanum of the ear. Fix your attention upon a particle of air as the sound-wave passes over it; it is urged from its position of rest towards a neighbour particle, first with an accelerated motion, and then with a retarded one. The force which first urges it is opposed by the resilience of the air, which finally stops the particle and causes it to recoil. At a certain point of its excursion the velocity of the particle is at its maximum. *The intensity of the sound is proportional to the square of this maximum velocity.*

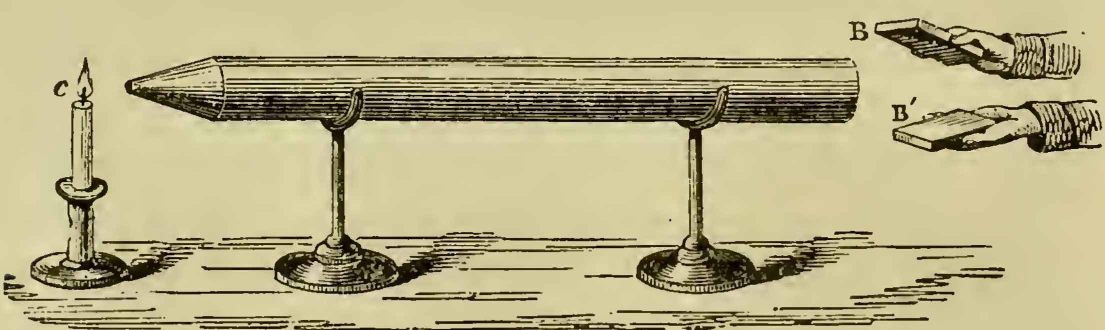
The distance through which the air-particle moves to and fro, when the sound-wave passes it, is called the *amplitude* of the vibration. The intensity of the sound is proportional to the square of the amplitude.

§ 4. *Confinement of Sound-waves in Tubes.*

This weakening of the sound, according to the law of inverse squares, would not take place if the sound-wave were so confined as to prevent its lateral diffusion. By sending it through a tube with a smooth interior surface

we accomplish this, and the wave thus confined may be transmitted to great distances with very little diminution of intensity. Into one end of a tin tube, fifteen feet long, I whisper in a manner quite inaudible to the people nearest to me, but a listener at the other end hears me distinctly. If a watch be placed at one end of the tube, a person at the other end hears the ticks, though nobody else does. At the distant end of the tube is now placed a lighted candle, *c*, fig. 5. When the hands are clapped at this end, the flame instantly ducks down at the other. It is not quite extinguished, but it is forcibly depressed.

Fig. 5.



When two books, B B′, fig. 5, are clapped together, the candle is blown out.[1] You may here observe, in a rough way, the speed with which the sound-wave is propagated. The instant the clap is heard the flame is extinguished. I do not say that the time required by the sound to travel through this tube is immeasurably short, but simply that the interval is too short for you to appreciate it.

That it is a *pulse* and not a *puff* of air is proved by filling one end of the tube with the smoke of brown paper. On clapping the books together no trace of this smoke is ejected from the other end. The pulse has passed through both smoke and air without carrying either of them along with it.

An effective mode of throwing the propagation of a

[1] To converge the pulse upon the flame, the tube was caused to end in a cone.

pulse through air has been devised by my assistant. The two ends of a tin tube fifteen feet long are stopped by sheet india-rubber stretched across them. At one end, *e*, a hammer with a spring handle rests against the india rubber; at the other end is an arrangement for the striking of a bell, *c*. Drawing back the hammer *e* to a distance measured on the graduated circle *f*, and liberating it, the generated pulse is propagated through the tube, strikes the other end, drives away the cork termination *a* of the lever *a b*, and causes the hammer *b* to strike the bell

Fig. 6.

The rapidity of propagation is well illustrated here. When hydrogen (sent through the india-rubber tube H) is substituted for air, the bell does not ring.

The celebrated French philosopher, Biot, observed the transmission of sound through the empty waterpipes of Paris, and found that he could hold a conversation in a low voice through an iron tube 3,120 feet in length. The lowest possible whisper, indeed, could be heard at this distance, while the firing of a pistol into one end of the tube quenched a lighted candle at the other.

§ 5. *The Reflection of Sound. Resemblances to Light.*

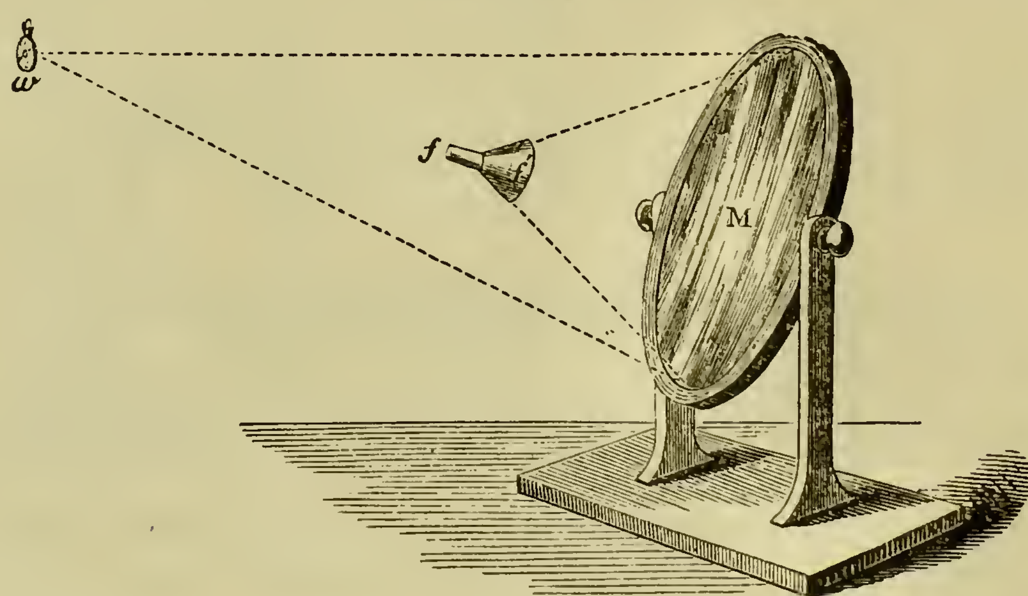
25 May

The action of sound thus illustrated is exactly the same as that of light and radiant heat They, like sound, are

for instance the heat that is radiated from a black stove

wave motions. Like sound, they diffuse themselves in space, diminishing in intensity according to the same law. Like sound also, light and radiant heat, when sent through a tube with a reflecting interior surface, may be conveyed to great distances with comparatively little loss. In fact, every experiment on the reflection of light has its analogy in the reflection of sound. On the gallery stands an electric lamp, placed close to a clock. An assistant in the gallery ignites the lamp, and directs its powerful beam upon a mirror placed behind the lecture table. By the

Fig. 7.

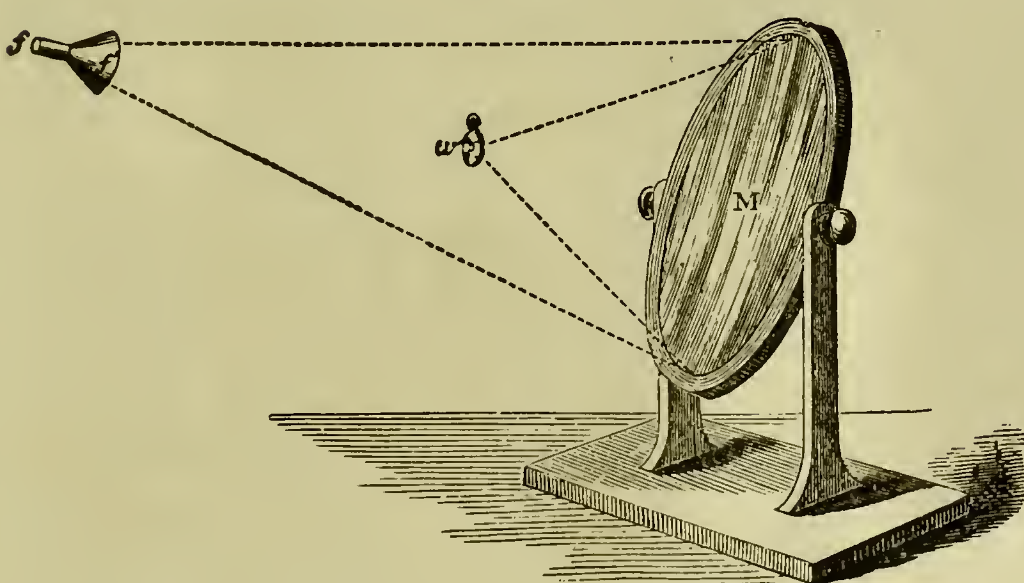


act of reflection the divergent beam is converted into a luminous cone traced out upon the dust of the room. The point of convergence being marked and the lamp extinguished, I place my ear at that point. The sound, like the light, is gathered up by the mirror, and the ticks are heard as if they came, not from the clock, but from the reflecting surface. Let us stop the clock, and place a watch, *w*, fig. 7, at the place occupied a moment ago by the electric light. At this great distance the ticking of the watch is distinctly heard. The hearing is much aided by introducing the end *f* of a glass funnel into the ear, the funnel here acting the part of an ear-trumpet. We know, moreover,

that in optics the positions of a body and of its image are reversible. When a candle is placed at this lower focus, you see its image on the gallery above, and I have only to turn the mirror on its stand to make the image of the flame fall upon any one of the row of persons who occupy the front seat in the gallery. Removing the candle, and putting the watch, *w*, fig. 8, in its place, the person on whom the light fell distinctly hears the sound.

The 'conjugate mirrors,' employed to illustrate the reflection of light and radiant heat, are also applicable here.

FIG. 8.

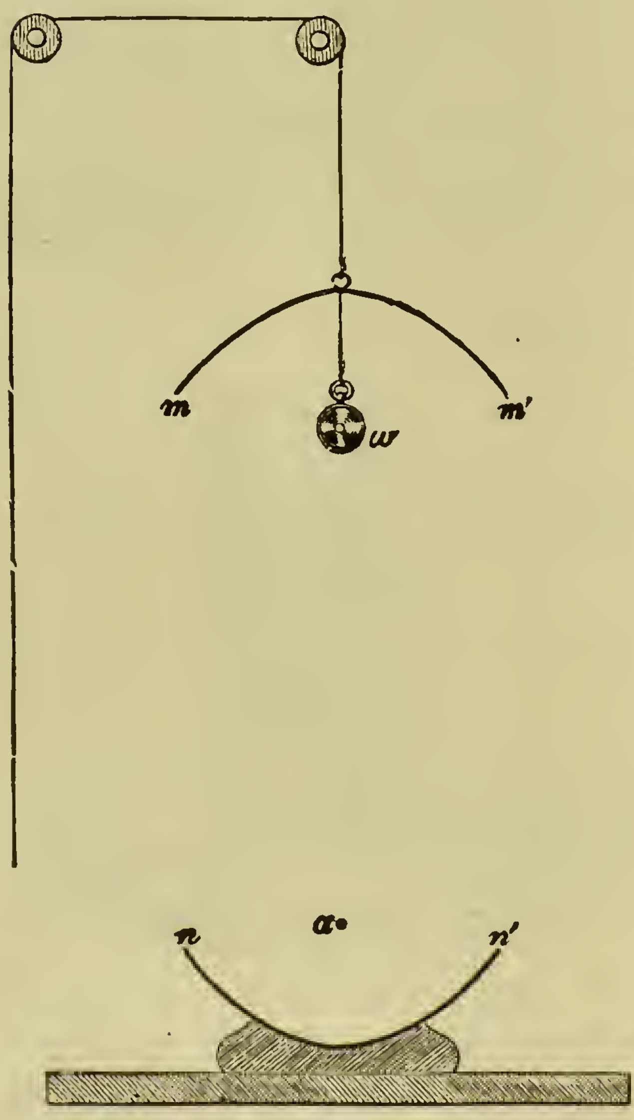


One of these two parabolic mirrors, *n n'*, fig. 9, is placed upon the table, the other, *m m'*, being drawn up to a height of five-and-twenty feet. When the electric light is placed in the focus *a* of the lower mirror, a parallel beam rises like a luminous pillar to the upper mirror, which brings the beam to a focus. At that focus is seen a spot of sunlike brilliancy, due to the reflection of the light from the surface of a watch, *w*. The watch is ticking, but I do not hear it. At this lower focus, *a*, however, we have the energy of every sonorous wave converged. Placing at *a* the ear, or better still a funnel with a tube reaching to the

ear, the ticking is as audible as if the watch were at hand; the sound, as in the former case, appearing to proceed, not from the watch itself, but from the lower mirror.[1]

Curved roofs and ceilings and bellying sails act as mirrors upon sound. Inconvenient secrets have been thus revealed, an instance of which has been cited by Sir John Herschel.[2] In one of the cathedrals in Sicily the confessional was so placed that the whispers of the penitents were reflected by the curved roof, and brought to a focus at a distant part of the edifice. The focus was discovered by accident, and for some time the person who discovered it took pleasure in hearing, and in bringing his friends to hear, utterances intended for the priest alone. One day, it is said, his own wife occupied the penitential stool, and both he and his friends were thus made acquainted with secrets which were the reverse of amusing to one of the party.

FIG. 9.



[1] It is recorded that a bell placed on an eminence in Heligoland failed, on account of its distance, to be heard in the town. A parabolic reflector placed behind the bell, so as to reflect the sound-waves in the direction of the long sloping street, caused the strokes of the bell to be distinctly heard at all times. This observation needs verification.

[2] *Ency. Met.* art. 'Sound.'

When a sufficient interval exists between a direct and a reflected sound, we hear the latter as an *echo.*

Sound, like light, may be reflected several times in succession, and as the reflected light under these circumstances becomes gradually feebler to the eye, so the successive echoes become gradually feebler to the ear. In mountain regions this repetition and decay of sound produce wonderful and pleasing effects. Visitors to Killarney will remember the fine echo in the Gap of Dunloe. When a trumpet is sounded at the proper place in the Gap, the sonorous waves reach the ear in succession after one, two, three, or more reflections from the adjacent cliffs, and thus die away in the sweetest cadences. There is a deep *cul-de-sac*, called the Ochsenthal, formed by the great cliffs of the Engelhörner, near Rosenlaui, in Switzerland, where the echoes warble in a wonderful manner. The sound of the Alpine horn, echoed from the rocks of the Wetterhorn or the Jungfrau, is in the first instance heard roughly. But by successive reflections the notes are rendered more soft and flute-like, the gradual diminution of intensity giving the impression that the source of sound is retreating further and further into the solitudes of ice and snow. The repetition of echoes is also in part due to the fact that the reflecting surfaces are at different distances from the hearer.

In large unfurnished rooms the mixture of direct and reflected sound sometimes produces very curious effects. Standing, for example, in the gallery of the Bourse at Paris, you hear the confused vociferation of the excited multitude below. You see all the motions—of their lips as well as of their hands and arms. You know they are speaking—often, indeed, with vehemence, but what they say you know not. The voices mix with their echoes into a chaos of noise, out of which no intelligible utterance can emerge. The echoes of a room are materially damped be

its furniture. The presence of an audience may also render intelligible speech possible where, without an audience, the definition of the direct voice is destroyed by its echoes On the 16th of May, 1865, having to lecture in the Senate House of the University of Cambridge, I first made some experiments as to the loudness of voice necessary to fill the room, and was dismayed to find that a friend placed at a distant part of the hall could not follow me because of the echoes. The assembled audience, however, so broke up and quenched the sonorous waves, that the echoes were practically absent, and my voice was plainly heard in all parts of the Senate House.

Sounds are also said to be reflected from the clouds. Arago reports that when the sky is clear the report of a cannon on an open plain is short and sharp, while a cloud is sufficient to produce an echo like the rolling of distant thunder. The subject of aerial echoes will be subsequently treated at length, when it will be shown that Arago's conclusion requires correction.

p 445

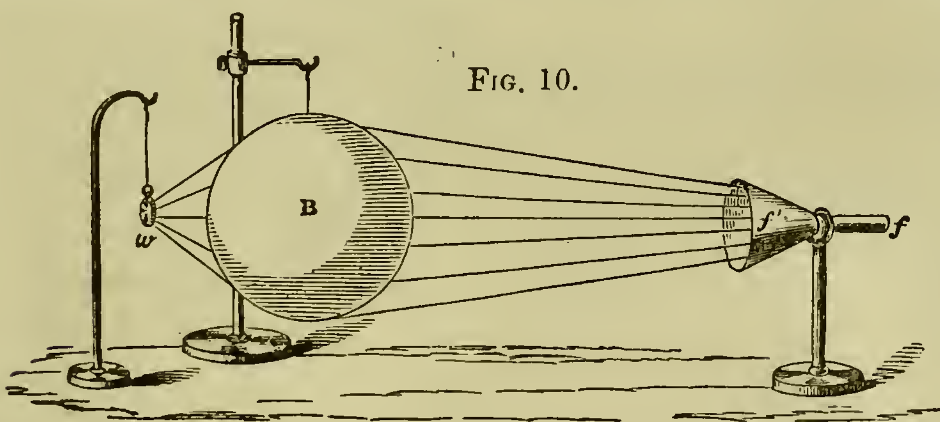
Sir John Herschel, in his excellent article 'Sound' in the 'Encyclopædia Metropolitana,' has collected among others the following instances of echoes. An echo in Woodstock Park repeats seventeen syllables by day and twenty by night; one on the banks of the Lago del Lupo, above the fall of Terni, repeats fifteen. The tick of a watch may be heard from one end of the abbey church of St. Albans to the other. In Gloucester Cathedral, a gallery of an octagonal form conveys a whisper seventy-five feet across the nave. In the whispering gallery of St. Paul's, the faintest sound is conveyed from one side to the other of the dome, but is not heard at any intermediate point. At Carisbrook Castle, in the Isle of Wight, is a well 210 feet deep and 12 wide. The interior is lined with smooth plaster; when a pin is dropped into the well it is distinctly heard to strike the water. Shout-

ing or coughing into this well produces a resonant ring of some duration.[1]

§ 6. *Refraction of Sound.*

Another important analogy between sound and light has been established by M. Sondhauss.[2] A large lens compels the solar rays that fall upon it to deviate from their direct and parallel course, and to form a convergent cone behind it. This refraction of the luminous beam is a consequence of the retardation suffered by the light in

FIG. 10.



passing through the glass. Sound may be similarly refracted by causing it to pass through a lens which retards its motion. Such a lens is formed when a thin balloon, which yields readily to the pulses striking against it, is filled with some gas heavier than air. A collodion, or a thin india-rubber balloon, B, fig. 10, filled with carbonic acid gas, answers this purpose. A watch, *w*, is hung up close to the lens, beyond which is placed the ear, assisted by the glass funnel *f f′*. By moving the head

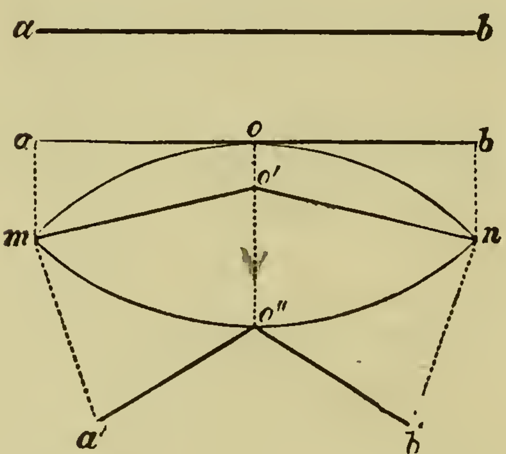
[1] Placing himself close to the upper part of the wall of the London Colosseum, a circular building 130 feet in diameter, Sir Charles Wheatstone found a word pronounced to be repeated a great many times. A single exclamation appeared like a peal of laughter, while the tearing of a piece of paper was like the patter of hail.

[2] *Poggendorff's Annalen*, vol. lxxxv. p. 378; *Philosoph. Mag.* vol. v. p. 73.

about, a position is soon discovered in which the ticking is particularly loud. This is the focus of the lens. If the ear be moved from this focus the intensity of the sound ~~falls~~ diminishes ; if, when the ear is at the focus, the balloon be removed, the ticks are weakened ; on replacing the balloon their force is restored. The lens, in fact, enables us to hear the ticks distinctly when they are perfectly inaudible to the unaided ear.[1]

How a sound-wave is thus converged may be comprehended by reference to fig. 11. Let $m\, o\, n\, o''$ be a section of the sound-lens, and $a\, b$ a portion of a sonorous wave approaching it from a distance. The middle point, o, of the wave first touches the lens, and is first retarded by it. By the time the ends a and b, still moving through air, reach the balloon, the middle point o, pursuing its way through the heavier gas within, will have only reached o'.

Fig. 11.



The wave is therefore broken at o' ; and the direction of motion being at right angles to the face of the wave, the two halves will encroach upon each other. This convergence of the two halves of the wave is augmented on quitting the lens. For when o' has reached o'', the two ends a and b will have pushed forward to a greater distance, say to a' and b'. Soon afterwards the two halves of the wave will cross each other, or in other words come to a focus, the air at the focus being agitated by the sum of the motions of the two waves.[2]

cf p 264 [1] A more complete mode of illustrating the refraction of sound is described in Lecture VI.

[2] For the sake of simplicity, the wave is shown broken at o', with its two halves straight. The surface of the wave, however, is really a curve, with its concavity turned in the direction of its propagation.

§ 7. *Diffraction of Sound: illustrations offered by great Explosions.*

When a long sea-roller meets an isolated rock in its passage, it rises against the rock and embraces it all round. Facts of this nature caused Newton to reject the undulatory theory of light. He contended that if light were a product of wave motion we could have no shadows, because the waves of light would propagate themselves round opaque bodies, as a wave of water round a rock. It has been proved since his time that the waves of light do bend round opaque bodies; but with that we have nothing now to do. A sound-wave certainly bends thus round an obstacle, though as it diffuses itself in the air at the back of the obstacle it is enfeebled in power, the obstacle thus producing a partial *shadow* of the sound. A railway train passing through cuttings and along embankments exhibits great variations in the intensity of the sound. The interposition of a hill in the Alps suffices to diminish materially the sound of a cataract; it is able sensibly to extinguish the tinkle of the cow-bells. Still the sound-shadow is but partial, and the marker at the rifle butts never fails to hear the explosion, though he is well protected from the ball. A striking example of this *diffraction* of a sonorous wave was exhibited at Erith after the tremendous explosion of a powder magazine which occurred there in 1864. The village of Erith was some miles distant from the magazine, but in nearly all cases the windows were shattered; and it was noticeable that the windows turned away from the origin of the explosion suffered almost as much as those which faced it. Lead sashes were employed in Erith Church, and these, being in some degree flexible, enabled the windows to yield to pressure without much fracture of the glass. As the sound-wave reached the church it separated right and left,

and, for a moment, the edifice was clasped by a girdle of intensely compressed air, every window in the church, front and back, being bent *inwards*. After compression, the air within the church no doubt dilated, tending to restore the windows to their first condition. The bending in of the windows, however, produced but a small condensation of the whole mass of air within the church; the recoil was therefore feeble in comparison with the pressure, and insufficient to undo what the latter had accomplished.[1]

§ 8. *Velocity of Sound: relation to Density and Elasticity of Air.*

3rd June

Two conditions determine the velocity of propagation of a sonorous wave: namely, the elasticity and the density of the medium through which the wave passes. The elasticity of air is measured by the pressure which it sustains or can hold in equilibrium. At the sea-level this pressure is equal to that of a stratum of mercury about 30 inches high. At the summit of Mont Blanc the barometric column is not much more than half this height; and, consequently, the elasticity of the air upon the summit of the mountain is not much more than half what it is at the sea-level.

If we could augment the elasticity of air, without at the same time augmenting its density, we should augment the velocity of sound. Or if, allowing the elasticity to

[1] The explosion of a powder-laden barge on the Regent's Park Canal a few years ago produced effects similar to those mentioned in § 7. The sound-wave bent round houses and broke the windows at the back, the coalescence of different portions of the wave at special points being marked by intensified local action. Close to the place where the explosion occurred the unconsumed gunpowder was in the wave, and as a consequence the dismantled gate-keeper's Lodge was girdled all round by a black belt of carbon.

remain constant, we could diminish the density, we should augment the velocity. Now, air in a closed vessel, where it cannot expand, has its elasticity augmented by heat, while its density remains unchanged. Through such heated air sound travels more rapidly than through cold air. Again, air free to expand has its density lessened by warming, its elasticity remaining the same, and through such air sound travels more rapidly than through cold air. This is the case with our atmosphere when heated by the sun.

The velocity of sound in air, *at the freezing temperature*, is 1,090 feet a second.

At all lower temperatures the velocity is less than this, and at all higher temperatures it is greater. The late M. Wertheim has determined the velocity of sound in air of different temperatures, and here are some of his results:—

Temperature of air.	Velocity of sound.
0·5° centigrade	1089 feet
2·10 ,,	1091 ,,
8·5 ,,	1109 ,,
12·0 ,,	1113 ,,
26·6 ,,	1140 ,,

At a temperature of half a degree above the freezing point of water the velocity is 1,089 feet a second; at a temperature of 26·6 degrees, it is 1,140 feet a second, or a difference of 51 feet for 26 degrees—that is to say, an augmentation of velocity of nearly 2 feet for every degree centigrade.

With the same elasticity as air the density of hydrogen gas is much less than that of air, and the consequence is that the velocity of sound in hydrogen far exceeds its velocity in air. The reverse holds good for heavy carbonic acid gas. If density and elasticity vary in the same proportion, as the law of Boyle proves them to do in air

when the temperature is preserved constant, they neutralise each other's effect; hence, if the temperature were the same, the velocity of sound upon the summits of the highest Alps would be the same as at the mouth of the Thames. But, inasmuch as the air above is colder than that below, the actual velocity on the summits of the mountains is less than that at the sea-level. To express this result in stricter language, the velocity is *directly* proportional to the square root of the elasticity of the air; it is also *inversely* proportional to the square root of the density of the air. Consequently, as in air of a constant temperature elasticity and density vary in the same proportion, and act oppositely, the velocity of sound is not affected by a change of density, if unaccompanied by a change of temperature.

There is no mistake more common than to suppose the velocity of sound to be augmented by density. The mistake has arisen from a misconception of the fact, that in solids and liquids the velocity is greater than in gases. But it is the high elasticity of these bodies, *in relation to their density*, that causes sound to pass rapidly through them. Other things remaining the same, an augmentation of density always produces a diminution of velocity. Were the elasticity of water, (which is measured by its compressibility) only equal to that of air, the velocity of sound in water, instead of being more than quadruple the velocity in air, would be only a small fraction of that velocity. Both density and elasticity, then, must be always borne in mind; the velocity of sound being determined by neither taken separately, but by the relation of the one to the other. The effect of small density and high elasticity is exemplified in an astonishing manner by the luminiferous ether, which transmits the vibrations of light—not at the rate of so many feet, but at the rate of one hundred and eighty-six thousand miles a second.

As regards the determination of the velocity of sound in air, hours might be filled with a simple statement of the efforts made to establish it with precision. The question has occupied the attention of experimenters in England, France, Germany, Italy, and Holland. But to the French and Dutch philosophers we owe the application of the last refinements of experimental skill to the solution of the problem. They neutralised effectually the influence of the wind; they took into account barometric pressure, temperature, and hygrometric condition. Sounds were started at the same moment from two distant stations, and thus caused to travel from station to station through the selfsame air. The distance between the stations was determined by exact trigonometrical observations, and means were devised for measuring with the utmost accuracy the time required by the sound to pass from the one station to the other. This time, expressed in seconds, divided into the distance expressed in feet, gave 1,090 feet per second as the velocity of sound through air at the temperature of 0° centigrade. = 32° Fah or freezing point

The time required by light to travel over all terrestrial distances is practically zero; and in the experiments just referred to the moment of explosion was marked by the flash of a gun, the time occupied by the sound in passing from station to station being the interval observed between the appearance of the flash and the arrival of the sound. The velocity of sound in air being once established, it is plain that we can apply it to the determination of distances. By observing, for example, the interval between the appearance of a flash of lightning and the arrival of the accompanying thunder peal, we at once determine the distance of the place of discharge. It is only when the interval between the flash and peal is short that danger from lightning is to be apprehended.

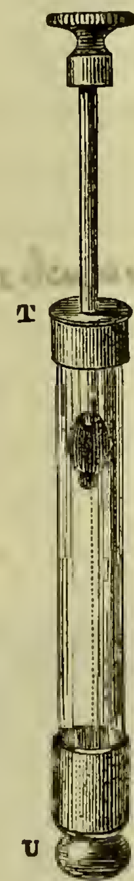
§ 9. *Theoretic Velocity calculated by Newton. Laplace's Correction.*

We now come to one of the most delicate points in the whole theory of sound. The velocity through air has been determined by direct experiment; but, knowing the elasticity and density of the air, it is possible, without any experiment at all, to calculate the velocity with which a sound-wave is transmitted through it. Sir Isaac Newton made this calculation, and found the velocity at the freezing temperature to be 916 feet a second. This is about one-sixth less than actual observation had proved the velocity to be, and the most curious suppositions were made to account for the discrepancy. Newton himself threw out the conjecture that it was only in passing from particle to particle of the air that sound required *time* for its transmission; that it moved instantaneously *through the particles themselves.* He then supposed the line along which sound passes to be occupied by air-particles for one-sixth of its extent, and thus he sought to make good the missing velocity. The very art and ingenuity of this assumption were sufficient to throw doubt on it; other theories were therefore advanced, but the great French mathematician Laplace was the first to completely solve the enigma. I shall now endeavour to make you thoroughly acquainted with his solution.

Into this strong cylinder of glass, T U, fig. 12, which is accurately bored, and quite smooth within, fits an air-tight piston. By pushing the piston down, I condense the air beneath it, heat being at the same time developed. A scrap of amadou attached to the bottom of the piston is ignited by the heat generated by compression. If a bit of cotton wool dipped into bisulphide of carbon be attached to the piston, when the latter is forced down, a flash of light, due to the ignition of the bisulphide of

carbon vapour, is observed within the tube. It is thus proved that when air is compressed heat is generated. By another experiment it may be shown that when air is rarefied cold is developed. This iron box contains a quantity of air. I open the cock, and permit the condensed air to discharge itself against the bulb of a suitable thermometer; the sinking of the instrument immediately declares the chilling of the air.

Fig. 12.



All that you have heard regarding the transmission of a sonorous pulse through air is, I trust, still fresh in your minds. As the pulse advances it squeezes the particles of air together, and two results follow from this compression. Firstly, its elasticity is augmented through the mere augmentation of its density. Secondly, its elasticity is augmented by the heat of compression. It was the change of elasticity resulting from a change of density, and that only, which Newton took into account. But, over and above the elasticity involved in Newton's calculation, we have an additional elasticity due to changes of temperature produced by the sound-wave itself. When both are taken into account, the calculated and the observed velocities agree perfectly.

But here, without due caution, we may fall into the gravest error. In fact, in dealing with nature, the mind must be on the alert to seize all her conditions; otherwise we soon learn that our thoughts are not in accordance with her facts. It is to be particularly noted that the augmentation of velocity due to the changes of temperature produced by the sonorous wave itself is totally different from the augmentation arising from the heating of the general mass of the air. The *average* temperature of the air is unchanged by the waves of sound. We cannot have

a condensed pulse without having a rarefied one associated with it. But in the rarefaction the temperature of the air is as much lowered as it is raised in the condensation. Supposing then the atmosphere parcelled out into such condensations and rarefactions, with their respective temperatures, an extraneous sound passing through such an atmosphere would be as much retarded in the latter as accelerated in the former, and no variation of the average velocity could result from such a distribution of temperature.

Whence then does the augmentation pointed out by Laplace arise? I would ask your best attention while I endeavour to make this knotty point clear to you. If air

Fig. 13.

be compressed it becomes smaller in volume; if the pressure be diminished the volume expands. The force which resists compression, and which produces expansion, is the elastic force of the air. Thus an external pressure squeezes the air-particles together; their own elastic force holds them asunder, and the particles are in equilibrium when these two forces are in equilibrium. Hence it is that the external pressure is a measure of the elastic force. Let the middle row of dots, fig. 13, represent a series of air-particles in a state of quiescence between the points *a* and *x*. Then, because of the elastic force exerted between the particles, if any one of them be urged from its position of rest, the motion will be transmitted through the entire

series. Supposing the particle a to be driven by the prong of a tuning-fork, or some other vibrating body, towards x, so as to be caused finally to occupy the position a' in the lowest row of particles. At the instant the excursion of a commences, its motion begins to be transmitted to b. In the next following moments b transmits the motion to c, c to d, d to e, and so on. So that by the time a has reached the position a', the motion will have been propagated to some point o' of the line of particles more or less distant from a'. The entire series of particles between a' and o' is then in a state of condensation. The distance a' o', over which the motion has travelled during the excursion of a to a', will depend upon the elastic force exerted between the particles. Fix your attention on any two of the particles, say a and b. The elastic force between them may be figured as a spiral spring, and it is plain that the more flaccid this spring the more sluggish would be the communication of the motion from a to b; while the stiffer the spring the more prompt would be the communication of the motion. What is true of a and b is true for every other pair of particles between a and o. Now, the spring between every pair of these particles *is suddenly stiffened* by the heat developed along the line of condensation, and hence the velocity of propagation is augmented by this heat. Reverting to our old experiment with the row of boys, it is as if, by the very act of pushing his neighbour, the muscular rigidity of each boy's arm was increased, thus enabling him to deliver his push more promptly than he would have done without this increase of rigidity. The *condensed* portion of a sonorous wave is propagated in the manner here described, and it is plain that the velocity of propagation is augmented by the heat developed in the condensation.

Let us now turn our thoughts for a moment to the propagation of the rarefaction. Supposing, as before, the

middle row $a\ x$ to represent the particles of air in equilibrium under the pressure of the atmosphere, and suppose the particle a to be suddenly drawn to the right, so as to occupy the position a'' in the highest line of dots: a'' is immediately followed by b'', b'' by c'', c'' by d'', d'' by e''; and thus the rarefaction is propagated backward towards x'', reaching a point o'' in the line of particles by the time a has completed its motion to the right. Now, why does b'' follow a'' when a'' is drawn away from it? Manifestly because the elastic force exerted between b'' and a'' is then less than that between b'' and c''. In fact, b'' will be driven after a'' by a force equal to the difference of the two elasticities between a'' and b'' and between b'' and c''. The same remark applies to the motion of c'' after b'', to that of d'' after c'': in fact, to the motion of each succeeding particle when it follows its predecessor. The greater the difference of elasticity on the two sides of any particle the more promptly will it follow its predecessor. And here observe what the *cold* of rarefaction accomplishes. In addition to the diminution of the elastic force between a'' and b'' by the withdrawal of a'' to a greater distance, there is a further diminution due to the lowering of the temperature. *The cold developed augments the difference of elastic force on which the propagation of the rarefaction depends.* Thus we see that because the heat developed in the condensation augments the rapidity of propagation of the condensation, and because the cold developed in the rarefaction augments the rapidity of propagation of the rarefaction, the sonorous wave, which consists of a condensation and a rarefaction, must have its velocity augmented by the heat *and the cold* which it develops during its own progress.

It is worth while fixing your attention here upon the fact that the distance $a'\ o'$ to which the motion has been propagated while a is moving to the position a' may be

vastly greater than that passed over in the same time by the particle itself. The excursion of a' may be of microscopic minuteness, while the distance to which the motion is transferred during the time required by a' to perform this small excursion may be many feet, or even many yards.

§ 10. *Ratio of Specific Heats of Air deduced from Velocity of Sound.*

Having grasped this, even partially, I will ask you to accompany me to a remote corner of the domain of physics, with the view, however, of showing that remoteness does not imply discontinuity. Let a certain quantity of air at a temperature of 0°, contained in a perfectly inexpansible vessel, have its temperature raised 1°. Let the same quantity of air, placed in a vessel which permits the air to expand when it is heated—the pressure on the air being kept constant during its expansion—also have its temperature raised 1°. The quantities of heat employed in the two cases are different.[1] The one quantity expresses what is called the specific heat of air at constant volume; the other the specific heat of air at constant pressure. It is an instance of the manner in which apparently unrelated natural phenomena are bound together, that from the calculated and observed velocities of sound in air we can deduce the ratio of these two specific heats. Squaring Newton's theoretic velocity, squaring also the observed velocity, and dividing the greater square by the less, we obtain the ratio referred to. Calling the specific heat at constant volume C^v, and that at constant pressure C^p; calling, moreover, Newton's calculated velocity V, and the observed velocity V′, Laplace proved that

$$\frac{C^p}{C^v} = \frac{V'^2}{V^2}$$

[1] See *Heat a Mode of Motion*, Sixth ed. Lect. V.

Inserting the values of V and V′ in this equation, and making the calculation, we find

$$\frac{C_p}{C_v} = 1{\cdot}42.$$

Thus, without knowing either the specific heat at constant volume or at constant pressure, Laplace found the ratio of the greater of them to the less to be 1·42. It is evident from the foregoing formulæ, that the calculated velocity of sound, multiplied by the square root of this ratio, gives the observed velocity.

But there is one assumption connected with the determination of this ratio, which must be here brought clearly forth. It is assumed that the heat developed by compression *remains in the condensed portion of the wave*, and applies itself there to augment the elasticity; that no portion of it is lost by radiation. If air were a powerful radiator, this assumption could not stand. The heat developed in the condensation could not then remain in the condensation. It would radiate all round, lodging itself for the most part in the chilled and rarefied portion of the wave, which would be gifted with a proportionate power of absorption. Hence the direct tendency of radiation would be to equalise the temperatures of the different parts of the wave, and thus to abolish the increase of velocity which called forth Laplace's correction.[1]

§ 11. *Mechanical Equivalent of Heat deduced from Velocity of Sound.*

The question, then, of the correctness of this ratio involves the other and apparently incongruous question,

[1] In fact, the prompt abstraction of the motion of heat from the condensation, and its prompt communication to the rarefaction by the contiguous luminiferous ether, would prevent the former from ever rising so high, or the latter from ever falling so low, in temperature as it would do if the power of radiation were absent.

whether atmospheric air possesses any sensible radiative power. If the ratio be correct, the practical absence of radiative power on the part of air is so far demonstrated. How, then, are we to ascertain whether the ratio is correct or not? By a process of reasoning which illustrates still further how natural agencies are intertwined. It was this ratio, looked at by a man of genius, named Mayer, which helped him to a clearer and a grander conception of the relation and interaction of the forces of inorganic and organic nature than any philosopher up to his time had attained. Mayer was the first to show that the excess 0·42 of the specific heat at constant pressure over that at constant volume was the quantity of heat consumed in the work performed by the expanding gas. Assuming the air to be confined laterally and to expand in a vertical direction, in which case it would simply have to lift the weight of the atmosphere, he attempted to calculate the precise amount of heat consumed in the raising of this or any other weight. He thus sought to determine the 'mechanical equivalent' of heat. In the combination of his data his mind was clear, but for the numerical correctness of these data he was obliged to rely upon the experimenters of his age. Their results, though approximately correct, were not *so* correct as the transcendent experimental ability of Regnault, aided by the last refinements of constructive skill, afterwards made them. Without changing in the slightest degree the method of his thought or the form of his calculation, the simple introduction of the exact numerical data into the formula of Mayer brings out the true mechanical equivalent of heat.

But how are we able to speak thus confidently of the accuracy of this equivalent? We are enabled to do so by the labours of an Englishman, who worked at this subject contemporaneously with Mayer; and who, while animated by the creative genius of his illustrious German brother,

enjoyed also the opportunity of bringing the inspirations of that genius to the test of experiment. By the experiments of Mr. Joule, the mutual convertibility of mechanical work and heat was first conclusively established. And 'Joule's equivalent,' as it is rightly called, considering the amount of resolute labour and skill expended in its determination, is almost identical with that derived from the formula of Mayer.

§ 12. *Absence of Radiative Power of Air deduced from Velocity of Sound.*

Consider now the ground we have trodden, the curious labyrinth of reasoning and experiment through which we have passed. We started with the observed and calculated velocities of sound in atmospheric air. We found Laplace, by a special assumption, deducing from these velocities the ratio of the specific heat of air at constant pressure to its specific heat at constant volume. We found Mayer calculating from this ratio the mechanical equivalent of heat; finally, we found Joule determining the same equivalent by direct experiments on the friction of solids and liquids. And what is the result? Mr. Joule's experiments prove the result of Mayer to be the true one; they therefore prove the ratio determined by Laplace to be the true ratio; and, because they do this, they prove at the same time the practical absence of radiative power in atmospheric air. It seems a long step from the stirring of water, or the rubbing together of iron plates in Joule's experiments, to the radiation of the atoms of our atmosphere; both questions are, however, connected by the line of reasoning here followed out.

But the true physical philosopher never rests content with an inference when an experiment to verify or contravene it is possible. The foregoing argument is clenched

by bringing the radiative power of atmospheric air to a direct test. When this is done, experiment and reasoning are found to agree; air being proved to be a body sensibly devoid of radiative and absorptive power.[1]

But here the experimenter on the transmission of sound through gases needs a word of warning. In Laplace's day, and long subsequently, it was thought that gases of all kinds possessed only an infinitesimal power of radiation; but that this is not the case is now well established. It would be rash to assume that, in the case of such bodies as ammonia, aqueous vapour, sulphurous acid, and olefiant gas, their enormous radiative powers do not interfere with the application of the formula of Laplace. It behoves us to inquire whether the ratio of the two specific heats deduced from the velocity of sound in these bodies is the true ratio; and whether, if the true ratio could be found by other methods, its square root, multiplied into the calculated velocity, would give the observed velocity. From the moment heat first appears in the condensation and cold in the rarefaction of a sonorous wave in any of those gases, the radiative power comes into play to abolish the difference of temperature. The condensed part of the wave is on this account rendered more flaccid, and the rarefied part less flaccid than it would otherwise be, and with a sufficiently high radiative power the velocity of sound, instead of coinciding with that derived from the formula of Laplace, must approximate to that derived from the more simple formula of Newton.

§ 13. *Velocity of Sound through Gases, Liquids, and Solids.*

To complete our knowledge of the transmission of sound through gases, a table is here added from the excellent

[1] *Heat a Mode of Motion*, Lecture XII.

researches of Dulong, who employed in his experiments a method which shall be subsequently explained :—

VELOCITY OF SOUND IN GASES AT THE TEMPERATURE OF 0° C.

	Velocity
Air	1,092 feet.
Oxygen	1,040 „
Hydrogen	4,164 „
Carbonic acid	858 „
Carbonic oxide	1,107 „
Protoxide of nitrogen	859 „
Olefiant gas	1,030 „

According to theory, the velocities of sound in oxygen and hydrogen are inversely proportional to the square roots of the densities of the two gases. We here find this theoretic deduction verified by experiment. Oxygen being sixteen times heavier than hydrogen, the velocity of sound in the latter gas ought, according to the above law, to be four times its velocity in the former ; hence the velocity in oxygen being 1,040, in hydrogen calculation would make it 4,160. Experiment, we see, makes it 4,164.

The velocity of sound in liquids may be determined theoretically, as Newton determined its velocity in air ; for the density of a liquid is easily determined, and its elasticity can be measured by subjecting it to compression. In the case of water, the calculated and the observed velocities agree so closely as to prove that the changes of temperature produced by a sound-wave in water have no sensible influence upon the velocity. In a series of memorable experiments in the lake of Geneva, MM. Colladon and Sturm determined the velocity of sound through water, and made it 4,708 feet a second. By a mode of experiment which you will subsequently be able to comprehend, the late M. Wertheim determined the velocity through various liquids, and in the following table I have collected his results :—

TRANSMISSION OF SOUND THROUGH LIQUIDS.

Name of Liquid	Temperature	Velocity
		feet
River water (Seine)	15° C.	4,714
" "	30	5,013
" "	60	5,657
Sea water (artificial)	20	4,768
Solution of common salt	18	5,132
Solution of sulphate of soda . . .	20	5,194
Solution of carbonate of soda . . .	22	5,230
Solution of nitrate of soda . . .	21	5,477
Solution of chloride of calcium . .	23	6,493
Common alcohol	20	4,218
Absolute alcohol	23	3,804
Spirits of turpentine	24	3,976
Sulphuric ether	0	3,801

We learn from this table that sound travels with different velocities through different liquids; that a salt dissolved in water augments the velocity, and that the salt which produces the greatest augmentation is chloride of calcium. The experiments also teach us that in water, as in air, the velocity augments with the temperature. At a temperature of 15° C., for example, the velocity in Seine water is 4,714 feet, at 30° it is 5,013 feet and at 60° 5,657 feet a second

I have said that from the compressibility of a liquid, determined by proper measurements, the velocity of sound through the liquid may be deduced. Conversely, from the velocity of sound in a liquid the compressibility of the liquid may be deduced. Wertheim compared a series of compressibilities deduced from his experiments on sound with a similar series obtained directly by M. Grassi. The agreement of both, exhibited in the following table, is a strong confirmation of the accuracy of the method pursued by Wertheim:—

	Cubic compressibility	
	calculated from Wertheim's velocity of sound	from the direct experiments of M. Grassi
Sea water	0·0000467	0·0000436
Solution of common salt .	0·0000349	0·0000321
„ carbonate of soda	0·0000337	0·0000297
„ nitrate of soda .	0·0000301	0.0000295
Absolute alcohol . .	0·0000947	0·0000991
Sulphuric ether . .	0·0001002	0·0001110

The greater the resistance which a liquid offers to compression, the more promptly and forcibly will it return to its original volume after it has been compressed. The less the compressibility, therefore, the greater is the elasticity, and consequently, other things being equal, the greater the velocity of sound through the liquid.

We have now to examine the transmission of sound through solids. Here, as a general rule, the elasticity, as compared with the density, is greater than in liquids, and consequently the propagation of sound is more rapid. In the following table the velocity of sound through various metals, as determined by Wertheim, is recorded:—

VELOCITY OF SOUND THROUGH METALS.

Name of Metal	at 20° C.	at 100° C.	at 200° C.
Lead	4,030	3,951	—
Gold	5,717	5,640	5,619
Silver	8,553	8,658	8,127
Copper	11,666	10,802	9,690
Platinum	8,815	8,437	8,079
Iron	16,822	17,386	15,483
Iron wire (ordinary) . . .	16,130	16,728	—
Cast steel	16,357	16,153	15,709
Steel wire (English) . . .	15,470	17,201	16,394
Steel wire	16,023	16,443	—

As a general rule, the velocity of sound through metals is diminished by augmented temperature; iron is, however, a striking exception to this rule, but it is only within certain limits an exception. While, for example, a rise

of temperature from 20° to 100° C. in the case of copper causes the velocity to fall from 11,666 to 10,802, the same rise produces in the case of iron an increase of velocity from 16,882 to 17,386. Between 100° and 200°, however, we see that iron falls from the last figure to 15,483. In iron, therefore, up to a certain point the elasticity is augmented by heat; beyond that point it is lowered. Silver is also an example of the same kind.

The difference of velocity in iron and in air may be illustrated by the following instructive experiment: Choose one of the longest horizontal bars employed for fencing in Hyde Park; and let an assistant strike the bar at one end while the ear of the observer is held close to the bar at a considerable distance from the point struck. Two sounds will reach the ear in succession; the first being transmitted through the iron and the second through the air. This effect was obtained by M. Biot, in his experiments on the iron water-pipes of Paris. exp!

The transmission of sound through a solid depends on the manner in which the molecules of the solid are arranged. When the body is homogeneous, and without structure, sound is transmitted through it equally well in all directions. But this is not the case when the body, whether inorganic like a crystal or organic like a tree, possesses a definite structure. The same is also true of other things than sound. Subjecting, for example, a sphere of wood to the action of a magnet, it is not equally affected in all directions. It is repelled by the pole of the magnet, but it is most strongly repelled when the force acts along the fibre. Heat also is conducted with different facilities in different directions through wood. It is most freely conducted along the fibre, and it passes more freely across the ligneous layers than along them. Wood, therefore, possesses *three unequal axes* of calorific conduction. These, established by myself, coincide with the axes of

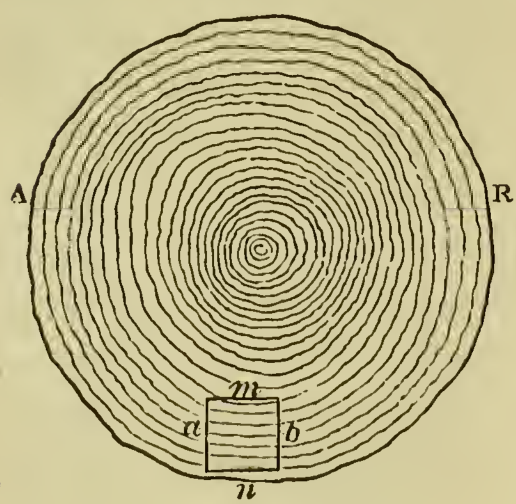
elasticity discovered by Savart. MM. Wertheim and Chevandier have determined the velocity of sound along these three axes and obtained the following results:—

VELOCITY OF SOUND IN WOOD.

Name of Wood	Along Fibre	Across Rings	Along Rings
Acacia	15,467	4,840	4,436
Fir	15,218	4.382	2,572
Beech	10,965	6 028	4,643
Oak	12,622	5,036	4,229
Pine	10,900	4,611	2,605
Elm	13,516	4,665	3,324
Sycamore	14,639	4,916	3,728
Ash	15.314	4,567	4,142
Alder	15,306	4,491	3,423
Aspen	16.677	5,297	2,987
Maple	13,472	5,047	3,401
Poplar	14,050	4,600	3,444

Separating a cube from the bark-wood of a good-sized tree, where the rings for a short distance may be regarded as straight; then if A R, fig. 14, be the section of the tree, the velocity of the sound in the direction *m n*, through such a cube, is greater than in the direction *a b*.

FIG. 14.



The foregoing table strikingly illustrates the influence of molecular structure. The great majority of crystals show differences of the same kind. Such bodies, for the most part, have their molecules arranged in different degrees of proximity in different directions, and where this occurs there are sure to be differences in the transmission and manifestation of heat, light, electricity, magnetism, and sound.

§ 14. *Hooke's anticipation of the Stethoscope.*

I will conclude this lecture on the transmission of sound through gases, liquids, and solids by a quaint and beautiful extract from the writings of that admirable thinker, Dr. Robert Hooke. It will be noticed that the philosophy of the stethoscope is enunciated in the following passage, and another could hardly be found passage which illustrates so well that action of the scientific imagination which, in all great investigators, is the precursor and associate of experiment:

'There may also be a possibility,' writes Hooke, 'of discovering the internal motions and actions of bodies by the sound they make. Who knows but that, as in a watch, we may hear the beating of the balance, and the running of the wheels, and the striking of the hammers, and the grating of the teeth, and multitudes of other noises; who knows, I say, but that it may be possible to discover the motions of the internal parts of bodies, whether animal, vegetable, or mineral, by the sound they make; that one may discover the works performed in the several offices and shops of a man's body, and thereby discover what instrument or engine is out of order, what works are going on at several times, and lie still at others, and the like; that in plants and vegetables one might discover by the noise the pumps for raising the juice, the valves for stopping it, and the rushing of it out of one passage into another, and the like? I could proceed further, but methinks I can hardly forbear to blush when I consider how the most part of men will look upon this: but, yet again, I have this encouragement, not to think all these things utterly impossible, though never so much derided by the generality of men, and never so seemingly mad, foolish, and phantastic, that as the thinking them impossible cannot much improve my knowledge, so the believing them possible may,

perhaps, be an occasion of taking notice of such things as another would pass by without regard as useless. And somewhat more of encouragement I have also from experience, that I have been able to hear very plainly the beating of a man's heart, and 'tis common to hear the motion of wind to and fro in the guts, and other small vessels; the stopping of the lungs is easily discovered by the wheezing, the stopping of the head by the humming and whistling noises, the slipping to and fro of the joints, in many cases, by crackling, and the like, as to the working or motion of the parts one amongst another; methinks I could receive encouragement from hearing the hissing noise made by a corrosive menstruum in its operation, the noise of fire in dissolving, of water in boiling, of the parts of a bell after that its motion is grown quite invisible as to the eye, for to me these motions and the other seem only to differ *secundum magis minus*, and so to their becoming sensible they require either that their motions be increased, or that the organ be made more nice and powerful to sensate and distinguish them.'

SUMMARY OF LECTURE I.

The sound of an explosion is propagated as a wave or pulse through the air.

This wave impinging upon the tympanic membrane causes it to shiver, its tremors are transmitted through the drum to the auditory nerve, and along the auditory nerve to the brain, where it announces itself as sound.

A sonorous wave consists of two parts, in one of which the air is condensed, and in the other rarefied.

The motion of the sonorous wave must not be confounded with the motion of the particles which at any moment form the wave. During the passage of the wave every particle concerned in its transmission makes only a small excursion to and fro.

The length of this excursion is called the *amplitude* of the vibration.

Sound cannot pass through a vacuum.

A certain sharpness of shock, or rapidity of vibration, is needed for the production of sonorous waves in air. It is still more necessary in hydrogen, because the greater mobility of this light gas tends to prevent the formation of condensations and rarefactions.

Sound is in all respects reflected like light; it is also refracted like light; and it may, like light, be condensed by suitable lenses.

Sound is also diffracted, the sonorous wave bending round obstacles; such obstacles, however, in part shade off the sound.

Echoes are produced by the reflected waves of sound.

In regard to sound and the medium through which it passes, four distinct things are to be borne in mind—intensity, velocity, elasticity, and density.

The intensity is proportional to the square of the amplitude as above defined.

It is also proportional to the square of the maximum velocity of the vibrating air-particles.

When sound issues from a *point* in free air, the intensity diminishes as the square of the distance from the point increases.

If the wave of sound be confined in a tube with a smooth interior surface, it may be conveyed to great distances without sensible loss of intensity.

The velocity of sound in air depends on the elasticity of the air in relation to its density. The greater the elasticity the swifter is the propagation: the greater the density the slower is the propagation.

The velocity is directly proportional to the square root of the elasticity; it is inversely proportional to the square root of the density

Hence, if elasticity and density vary in the same proportion, the one will neutralise the other as regards the velocity of sound

That they do vary in the same proportion is proved by the law of Boyle and Mariotte; hence the velocity of sound in air is independent of the density of the air.

But that this law shall hold good, it is necessary that the dense air and the rare air should have the same temperature.

The intensity of a sound depends upon the density of the air in which it is generated, but not on that of the air in which it is heard.

32° Fah. The velocity of sound in air of the temperature 0° C. is 1,090 feet a second; it augments nearly 2 feet for every degree centigrade added to its temperature.

59 Fah (15° C) 1090 ft per sec + 2×15 = 1120 ft per sec

√ Specific heat

Hence, given the velocity of sound in air, the temperature of the air may be readily calculated.

The distance of a fired cannon, or of a discharge of lightning, may be determined by observing the interval which elapses between the flash and the sound.

The pupil will find no difficulty in referring many common occurrences to the fact that sound requires a sensible time to pass through any considerable length of air. For example, the fall of the axe of a distant woodcutter is not simultaneous with the sound of the stroke. The flash of a distant gun always arrives before the sound. A company of soldiers marching to music along a road cannot march simultaneously ~~in perfect time~~, for the notes do not reach those in front and those behind simultaneously.

In the condensed portion of a sonorous wave the air is ~~above~~, in the rarefied portion of the wave it is ~~below~~, its average temperature.

This change of temperature, produced by the passage of the sound-wave itself, virtually augments the elasticity of the air, and makes the velocity of sound greater than it would be if there were no change of temperature.

The velocity found by Newton, who did not take this change of temperature into account, was 916 feet a second.

Laplace proved that by multiplying Newton's velocity by the square root of the ratio of the specific heat of air at constant pressure to its specific heat at constant volume, the actual or observed velocity is obtained.

Conversely, from a comparison of the calculated and observed velocities, the ratio of the two specific heats may be inferred.

The mechanical equivalent of heat may be deduced from this ratio; it is found to be the same as that established by direct experiment.

This coincidence leads to the conclusion that atmo-

spheric air is devoid of any sensible power to radiate heat. Direct experiments on the radiative power of air establish the same result.

The velocity of sound in water is more than four times its velocity in air.

The velocity of sound in iron is seventeen times its velocity in air.

The velocity of sound along the fibre of pine-wood is ten times its velocity in air.

The cause of this great superiority is that the elasticities of the liquid, the metal, and the wood, as compared with their respective densities, are vastly greater than the elasticity of air in relation to its density.

The velocity of sound is dependent to some extent upon molecular structure. In wood, for example, it is conveyed with different degrees of rapidity in different directions.

LECTURE II.

PHYSICAL DISTINCTION BETWEEN NOISE AND MUSIC—A MUSICAL TONE PRODUCED BY PERIODIC, NOISE PRODUCED BY UNPERIODIC, IMPULSES—PRODUCTION OF MUSICAL SOUNDS BY TAPS—PRODUCTION OF MUSICAL SOUNDS BY PUFFS—DEFINITION OF PITCH IN MUSIC—VIBRATIONS OF A TUNING FORK; THEIR GRAPHIC REPRESENTATION ON SMOKED GLASS—OPTICAL EXPRESSION OF THE VIBRATIONS OF A TUNING-FORK—DESCRIPTION OF THE SYREN—LIMITS OF THE EAR; HIGHEST AND DEEPEST TONES—RAPIDITY OF VIBRATION DETERMINED BY THE SYREN—DETERMINATION OF THE LENGTHS OF SONOROUS WAVES—WAVE-LENGTHS OF THE VOICE IN MAN AND WOMAN—TRANSMISSION OF MUSICAL SOUNDS THROUGH LIQUIDS AND SOLIDS.

§ 1. *Musical Sounds*

IN our last chapter we considered the propagation through air of a sound of momentary duration. We have to-day to consider continuous sounds, and to make ourselves in the first place acquainted with the physical distinction between noise and music. As far as sensation goes, everybody knows the difference between these two things. But we have now to inquire into the causes of sensation, and to make ourselves acquainted with the condition of the external air which in one case resolves itself into music and in another into noise.

We have already learned that what is loudness in our sensations is outside of us nothing more than width of swing, or *amplitude*, of the vibrating air-particles. Every other real sonorous impression of which we are conscious has its correlative without, as a mere form or state of the atmosphere. Were our organs sharp enough to see the motions of the air through which an agreeable voice is

passing, we might see stamped upon that air the conditions of motion on which the sweetness of the voice depends. In ordinary conversation, also, the physical precedes and arouses the psychical; the spoken language, which is to give us pleasure or pain, which is to rouse us to anger or soothe us to peace, existing for a time, between us and the speaker, as a purely mechanical condition of the intervening air.

Noise affects us as an irregular succession of shocks. We are conscious while listening to it of a jolting and jarring of the auditory nerve, while a musical sound flows smoothly and without asperity or irregularity. How is this smoothness secured? *By rendering the impulses received by the tympanic membrane perfectly periodic.* A periodic motion is one that repeats itself. The motion of a common pendulum, for example, is periodic, but its vibrations are far too sluggish to excite sonorous waves. To produce a musical tone we must have a body which vibrates with the unerring regularity of the pendulum, but which must impart much sharper and quicker shocks to the air.

Imagine the first of a series of pulses following each other at regular intervals, impinging upon the tympanic membrane. It is shaken by the shock; and a body once shaken cannot come instantaneously to rest. The human ear, indeed, is so constructed that the sonorous motion vanishes with extreme rapidity, but its disappearance is not instantaneous; and if the motion imparted to the auditory nerve by each individual pulse of our series continue until the arrival of its successor, the sound will not cease at all. The effect of every shock will be renewed before it vanishes, and the recurrent impulses will link themselves together to a continuous musical sound. The pulses, on the contrary, which produce noise are of irregular strength and recurrence. The action of noise upon the

ear has been well compared to that of a flickering light upon the eye, both being painful through the sudden and abrupt changes which they impose upon their respective nerves.

The only condition necessary to the production of a musical sound is that the pulses should succeed each other in the same interval of time. No matter what its origin may be, if this condition be fulfilled the sound becomes musical. If a watch, for example, could be caused to tick with sufficient rapidity—say one hundred times a second—the ticks would lose their individuality and blend to a musical tone. And if the strokes of a pigeon's wings could be accomplished at the same rate, the progress of the bird through the air would be accompanied by music. In the humming-bird the necessary rapidity is attained; and when we pass on from birds to insects, where the vibrations are more rapid, we have a musical note as the ordinary accompaniment of the insects' flight.[1] The puffs of a locomotive at starting follow each other slowly at first, but they soon increase so rapidly as to be almost incapable of being counted. If this increase could continue up to 50 or 60 puffs a second, the approach of the engine would be heralded by an organ peal of tremendous power.

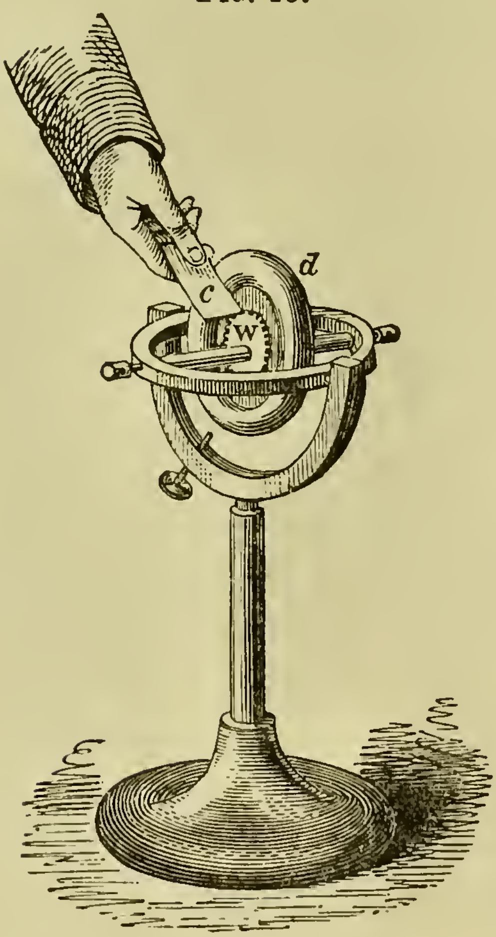
§ 2. *Musical Sounds produced by Taps.*

Galileo produced a musical sound by passing a knife over the edge of a piastre. The minute serration of the coin indicated the periodic character of the motion, which consisted of a succession of taps quick enough to produce sonorous continuity. Every schoolboy knows how to produce a note with his slate pencil. I will not call it musical, because this term is usually associated with pleasure, and the sound of the pencil is not pleasant.

[1] According to Burmeister, through the injection and ejection of air into and from the cavity of the chest.

The production of a musical sound by taps is usually effected by causing the teeth of a rotating wheel to strike in quick succession against a card. This was first illustrated by the celebrated Robert Hooke,[1] and nearer our own day by the eminent French experimenter Savart. We will confine ourselves to homelier modes of illustration.

FIG. 15.



This gyroscope is an instrument consisting mainly of a heavy brass ring *d*, fig. 15, loading the circumference of a disc, through which, and at right angles to its surface, passes a steel axis, delicately supported at its two ends. By coiling a string round the axis, and drawing it vigorously out, the ring is caused to spin rapidly; and along with it rotates a small toothed wheel *w*. On touching this wheel with the edge of a card *c*, a musical sound of exceeding shrillness is produced. I place my thumb for a moment against the ring; the rapidity of its rotation is thereby diminished, and this is instantly announced by a

[1] On July 27, 1681, 'Mr. Hooke showed an experiment of making musical and other sounds by the help of teeth of brass wheels; which teeth were made of equal bigness for musical sounds, but of unequal for vocal sounds.'—Birch's *History of the Royal Society*, p. 96, published in 1757.

The following extract is from the *Life of Hooke*, which precedes his *Posthumous Works*, published in 1705, by Richard Waller, Sec. R. S.:—'In July the same year he (Dr. Hooke) showed a way of making musical and other sounds by the striking of the teeth of several brass wheels, proportionally cut as to their numbers, and turned very fast round, in which it was observable that the equal or proportional stroaks of the teeth, that is, 2 to 1, 4 to 3, &c., made the musical notes, but the unequal stroaks of the teeth more answered the sound of the voice in speaking.'

lowering of the pitch of the note. By checking the motion still more, the pitch is lowered still further. We are here made acquainted with the important fact that the pitch of a note depends upon the rapidity of its pulses.[1] At the end of the experiment you hear the separate taps of the teeth against the card, their succession not being quick enough to produce that continuous flow of sound which is the essence of music. A screw with a milled head attached to a whirling table, and caused to rotate, produces by its taps against a card a note almost as clear and pure as that obtained from the toothed wheel of the gyroscope.

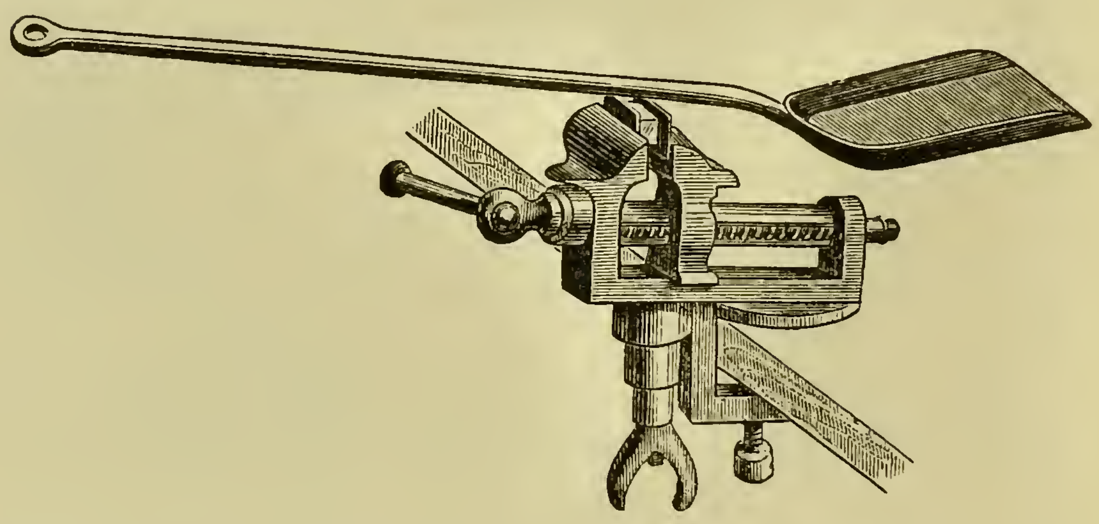
The production of a musical sound by taps may also be pleasantly illustrated in the following way: in this vice, fig. 16, are fixed vertically two pieces of sheet lead, with their horizontal edges a quarter of an inch apart. I lay a bar of brass across them, permitting it to rest upon the edges, and, tilting the bar a little, set it in oscillation like a see-saw. After a time, if left to itself, it comes to rest. But suppose the bar on touching the lead to be always tilted upwards by a force issuing from the lead itself, it is plain that the vibrations would then be rendered permanent. Now, such a force is brought into play *when the bar is heated.* On its then touching the lead heat is communicated, a sudden jutting upwards of the lead at the point of contact being the result. Hence an incessant tilting of the bar from side to side, so long as it continues sufficiently hot. Substituting for the brass bar the heated fire-shovel shown in fig. 16, the same effect is produced.

In its descent upon the lead the bar taps it gently, the taps being so slow that you may readily count them. But a mass of metal differently shaped may be caused to

[1] Galileo, finding the number of notches on his metal to be great when the pitch of the note was high, inferred that the pitch depended on the rapidity of the impulses.

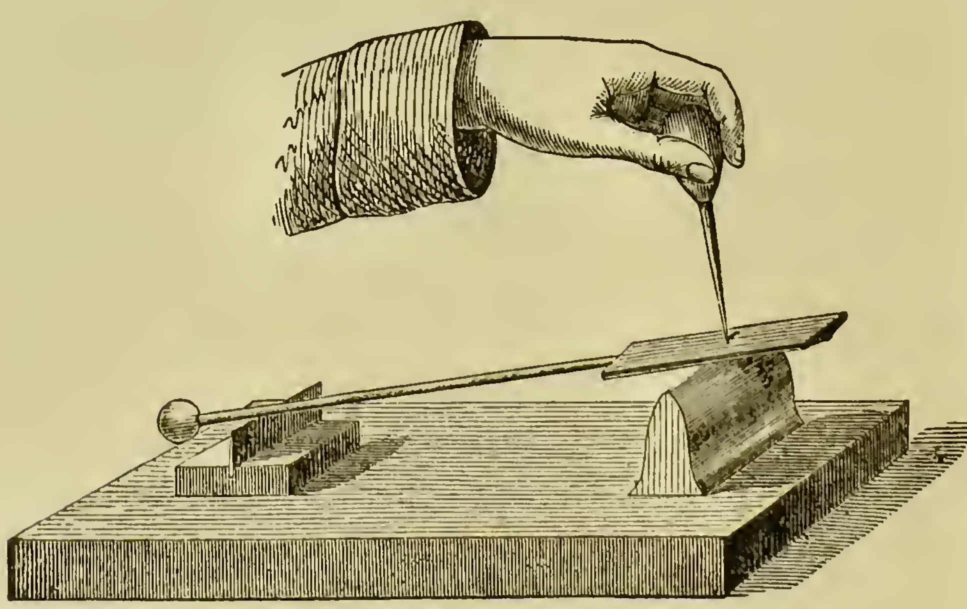
vibrate more briskly, and the taps to succeed each other more rapidly. When such a heated *rocker*, fig. 17, is placed upon a block of lead, the taps hasten to a loud rattle. When, with the point of a file, the rocker is

FIG. 16.



pressed against the lead, the vibrations are rendered more rapid, and the taps link themselves together to a deep musical tone. A second rocker, which oscillates more quickly than the last, produces music without any other

FIG. 17.



pressure than that due to its own weight. Pressing it, however, with the file, the pitch rises, until a note of singular force and purity fills the room. Relaxing the

pressure, the pitch instantly falls; resuming the pressure, it again rises; and thus by the alteration of the pressure we obtain great variations of tone. Nor are such rockers essential. Allowing one face of the clean square end of a heated poker to rest upon the block of lead, a rattle is heard; causing another face to rest upon the block, a clear musical note is obtained. The two faces have been bevelled differently by a file, so as to secure different rates of vibration.[1] This curious effect was discovered by Schwartz and Trevelyan.[2]

§ 3. *Musical Sounds produced by Puffs.*

Professor Robison was the first to produce a musical sound by a quick succession of *puffs* of air. His device was the first form of an instrument which will soon be introduced to you under the name of the *syren*. Robison describes his experiment in the following words: 'A stopcock was so constructed that it opened and shut the passage of a pipe 720 times in a second. The apparatus was fitted to the pipe of a conduit leading from the bellows to the wind-chest of an organ. The air was simply allowed to pass gently along this pipe by the opening of the cock. When this was repeated 720 times in a second, the sound *g in alt* was most smoothly uttered,

[1] When a rough tide rolls in upon a pebbled beach, as at Blackgang Chine or Freshwater Gate in the Isle of Wight, the rounded stones are carried up the slope by the impetus of the water, and when the wave retreats the pebbles are dragged down. Innumerable collisions thus ensue of irregular intensity and recurrence. The union of these shocks produces a kind of scream. Hence the line in Tennyson's *Maud*:—

'Now to the scream of a maddened beach dragged down by the wave.'

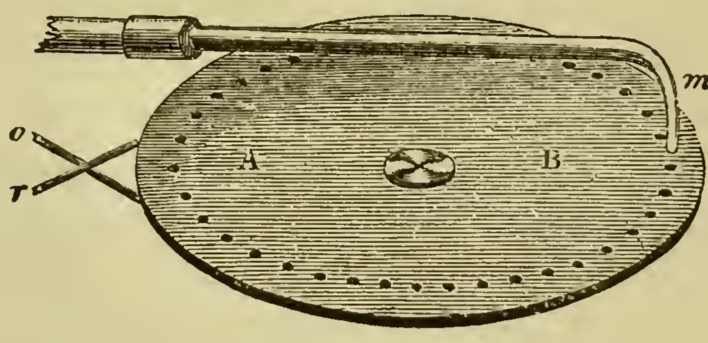
The height of the note depends in some measure upon the size of the pebbles, varying from a kind of roar—heard when the stones are large—to a scream; from a scream to a noise resembling that of frying bacon; and from this, when the pebbles are so small as to approach the state of gravel, to a mere hiss. The roar of the breaking wave itself is mainly due to the explosion of bladders of air.

Heat a Mode of Motion, Sixth ed., p. 96

equal in sweetness to a clear female voice. When the frequency was reduced to 360, the sound was that of a clear, but rather a harsh man's voice. The cock was now altered in such a manner that it never shut the hole entirely, but left about one-third of it open. When this was repeated 720 times in a second, the sound was uncommonly smooth and sweet. When reduced to 360, the sound was more mellow than any man's voice of the same pitch.'

But the difficulty of obtaining the necessary speed renders another form of the experiment preferable. A disc of Bristol board B, fig. 18, twelve inches in diameter,

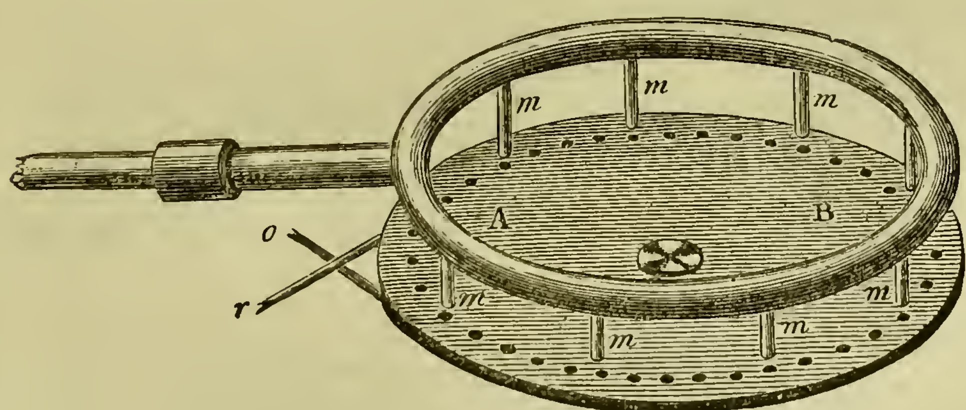
FIG. 18.



is perforated at equal intervals along a circle near its circumference. The disc, being strengthened by a backing of tin, can be attached to a whirling table and caused to rotate rapidly. The individual holes then disappear, blending themselves into a continuous shaded circle. Immediately over this circle is placed a bent tube *m*, connected with a pair of acoustic bellows. The disc is now motionless, the lower end of the tube being immediately over one of the perforations of the disc. If, therefore, the bellows be worked, the wind will pass from *m* through the hole underneath. But if the disc be turned a little, an unperforated portion of the disc comes under the tube, the current of air being then intercepted. As the disc is slowly turned successive perforations are brought under the tube, and whenever this occurs a puff

of air gets through. On rendering the rotation rapid, the puffs succeed each other in very quick succession, producing pulses in the air which blend to a continuous musical note, audible to you all. Mark how the note varies. When the whirling table is turned rapidly the sound is shrill; when its motion is slackened the pitch immediately falls. If instead of a single glass tube there were two of them, as far apart as two of our orifices, so that whenever the one tube stood over an orifice, the other should stand over another, it is plain that if both tubes were blown through, we should, on turning the disc, get

FIG. 19.



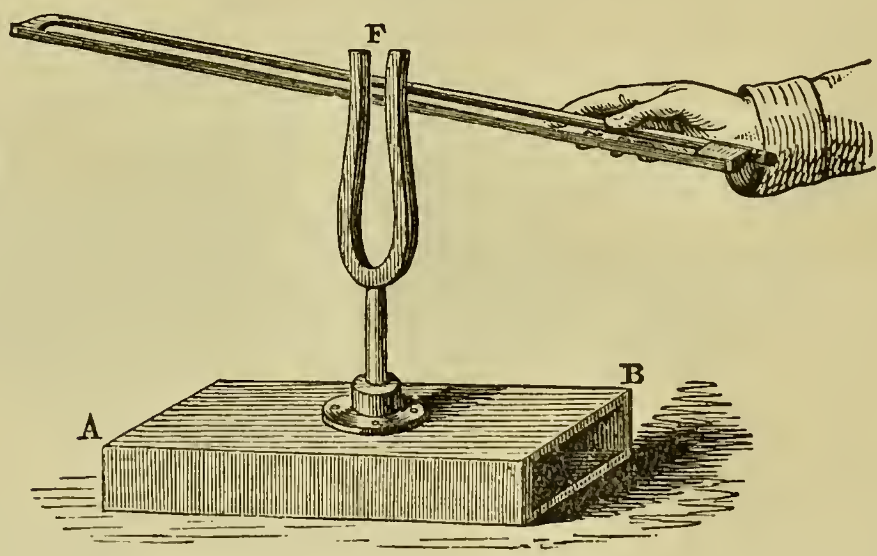
a puff through two holes at the same time. The intensity of the sound would be thereby augmented, but the pitch would remain unchanged. The two puffs issuing at the same instant would act in concert, and produce a greater effect than one upon the ear. And if instead of two tubes we had ten of them, or better still, if we had a tube for every orifice in the disc, the puffs from the entire series would all issue, and would all be cut off at the same time. These puffs would produce a note of far greater intensity than that obtained by the alternate escape and interruption of the air from a single tube. In the arrangement now before you, fig. 19, there are nine tubes through which the air is urged—through nine apertures, therefore, puffs escape at once. On turning the whirling table,

and alternately increasing and relaxing its speed, the sound rises and falls like the loud wail of a changing wind.

§ 4. *Musical Sounds produced by a Tuning-fork.*

Various other means may be employed to throw the air into a state of periodic motion. A stretched string pulled aside and suddenly liberated imparts vibrations to the air which succeed each other in perfectly regular intervals. A tuning-fork does the same. When a bow is drawn across the prongs of a tuning-fork, fig. 20, the

Fig. 20.

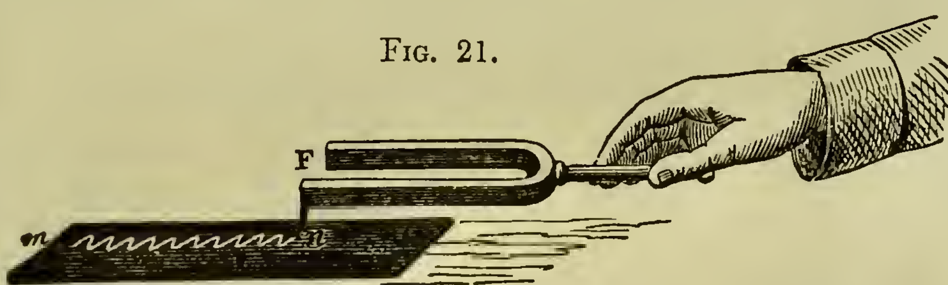


resin of the bow enables the hairs to grip the prong, which is thus pulled aside. But the resistance of the prong soon becomes too strong, and it starts suddenly back; it is, however, immediately laid hold of again by the bow, to start back once more as soon as its resistance becomes great enough. This rhythmic process, continually repeated during the passage of the bow, finally throws the fork into a state of intense vibration, and the result is a musical note. A person close at hand could see the fork vibrating; a deaf person bringing his hand sufficiently near would feel the shivering in the air. Or causing its vibrating prong to touch a card, the taps

against the card link themselves, as in the case of the gyroscope, to a musical sound, the fork coming rapidly to rest. What we call silence expresses this absence of motion.

When the tuning-fork is first excited the sound issues from it with maximum loudness, becoming gradually feebler as the fork continues to vibrate. A person close to the fork can notice at the same time that the amplitude, or space through which the prongs oscillate, becomes gradually less and less. But the most expert ear in this assembly can detect no change in the pitch of the note. The lowering of the intensity of a note does not therefore imply the lowering of its pitch. Though the amplitude

Fig. 21.



changes, the rate of vibration remains the same. Pitch and intensity must therefore be held distinctly apart: the latter depends solely upon the amplitude, the former solely upon the rapidity of vibration.

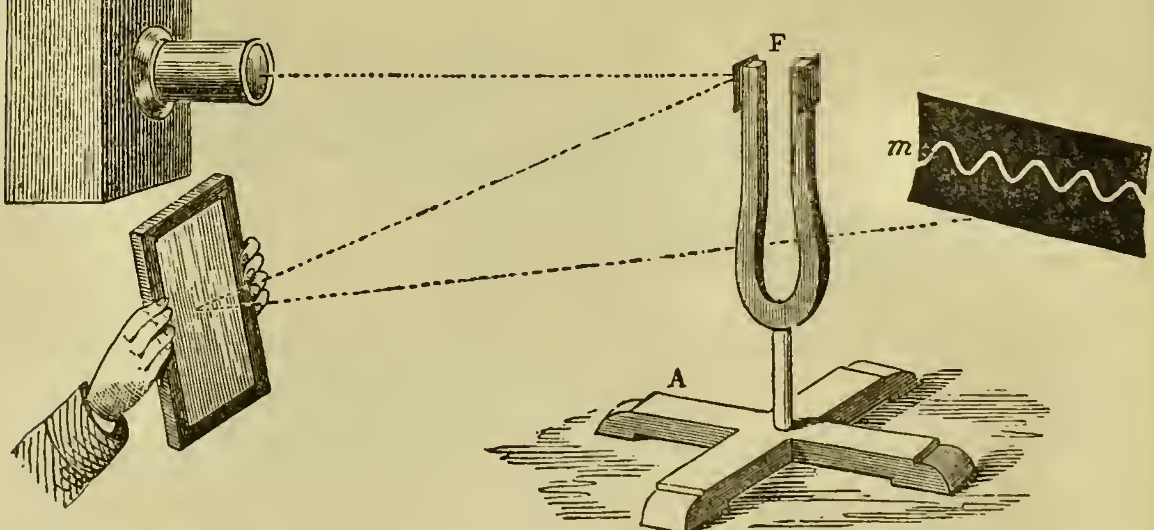
This tuning-fork may be caused to write the story of its own motion. Attached to the side of one of its prongs F, fig. 21, is a thin strip of sheet copper which tapers to a point. When the tuning-fork is excited it vibrates, and the strip of metal accompanies it in its vibration. The point of the strip being brought gently down upon a piece of smoked glass, it moves to and fro over the smoked surface, leaving a clear line behind. As long as the hand is kept motionless, the point merely passes to and fro over the same line; but it is plain that we have only to draw the fork along the glass to produce a sinuous line, *m n*, fig. 21.

When this process is repeated without exciting the

fork afresh, the depth of the indentations diminishes. The sinuous line approximates more and more to a straight one. This is the visual expression of decreasing amplitude. When the sinuosities entirely disappear the amplitude has become zero, and the sound, which depends upon the amplitude, ceases altogether.

To M. Lissajous we are indebted for a very beautiful method of giving optical expression to the vibrations of a tuning-fork. Attached to one of the prongs of a very large fork is a small metallic mirror, F, fig. 22, the other prong being loaded with a piece of metal to establish equi-

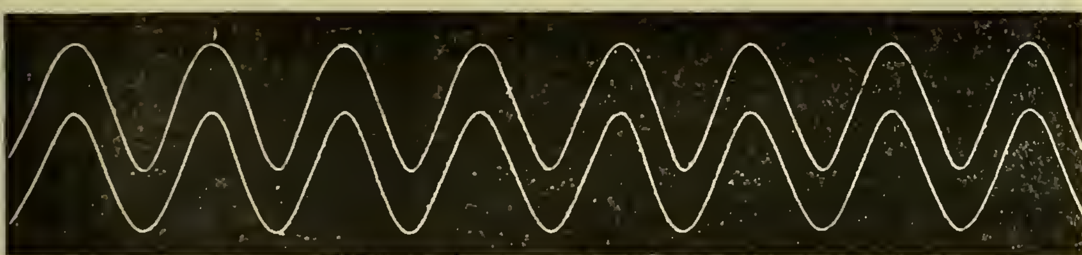
Fig. 22.



librium. Permitting a slender beam of intense light to fall upon the mirror, the beam is thrown back by reflection. In my hands is held a small looking-glass, which receives the reflected beam, and from which it is again reflected to the screen, forming a small luminous disc upon the white surface. The disc is perfectly motionless; but the moment the fork is set in vibration the reflected beam is tilted rapidly up and down, the disc describing a band of light two or three feet long. The length of the band depends on the amplitude of the vibration, and you see it gradually shorten as the motion of the fork is expended. It remains, however, a straight line as long as

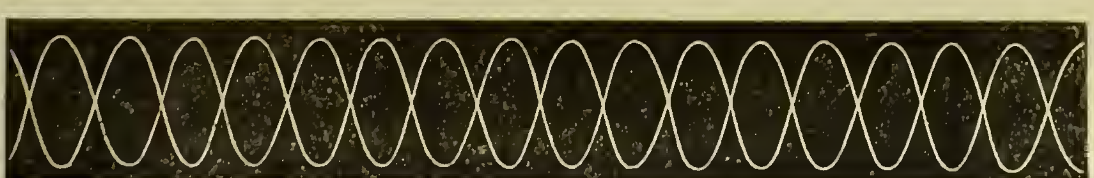
the glass is held in a fixed position. But on suddenly turning the glass so as to make the beam travel from left to right over the screen, you observe the straight line instantly resolved into a beautiful luminous ripple $m\ n$. A luminous impression once made upon the retina lingers there for a fraction of a second; and if then the time required to transfer the elongated image from side to side of the screen be less than this fraction, the wavy line of

Fig. 23.



light will occupy for a moment the whole width of the screen. Instead of permitting the beam from the lamp to issue through a single aperture, it may be caused to issue through two apertures, about half an inch asunder, two discs of light, one *above* the other, being thus projected upon the screen. When the fork is excited and the mirror turned, we have a brilliant double sinuous line running over the dark surface, fig. 23. Turning the diaphragm so as to place the two discs *beside* each other, on exciting

Fig. 24.



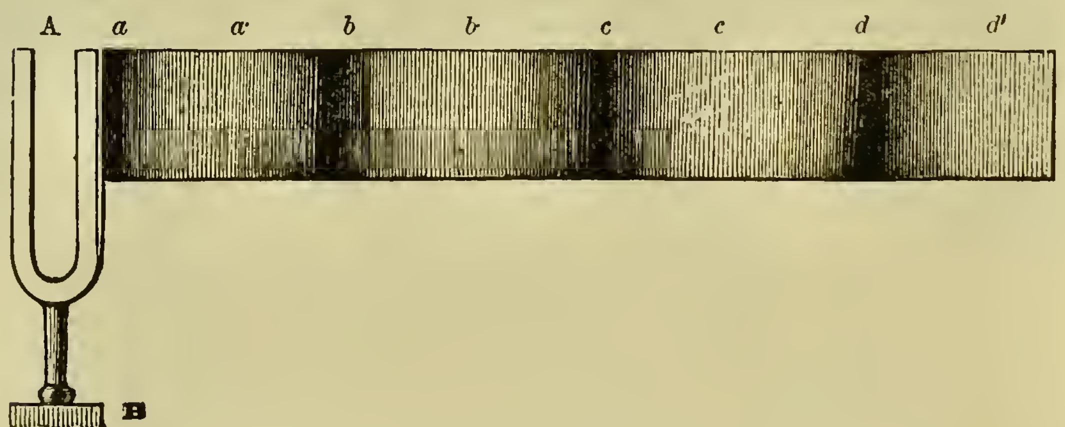
the fork and moving the mirror, we obtain a beautiful interlacing of the two sinuous lines, fig. 24.

§ 5. *The Waves of Sound.*

How are we to picture to ourselves the condition of the air through which this musical sound is passing? Imagine one of the prongs of the vibrating fork swiftly

advancing; it compresses the air immediately in front of it, and when it retreats it leaves a partial vacuum behind, the process being repeated by every subsequent advance and retreat. The whole function of the tuning-fork is to carve the air into these condensations and rarefactions, and they, as they are formed, propagate themselves in succession through the air. A condensation with its associated rarefaction constitutes, as already stated, a sonorous wave. In water the length of a wave is measured from crest to crest; while, in the case of sound, the *wave-length* is the distance between two successive condensa-

FIG. 25.



tions. The condensation of the sound-wave corresponds to the crest, while the rarefaction of the sound-wave corresponds to the *sinus*, or depression, of the water-wave. Let the dark spaces, *a*, *b*, *c*, *d*, fig. 25, represent the condensations, and the light ones, *a*′, *b*′, *c*′, *d*′, the rarefactions of the waves issuing from the fork A B: the wave-length would then be measured from *a* to *b*, from *b* to *c*, or from *c* to *d*.

§ 6. *Definition of Pitch: determination of Rates of Vibration.*

When two notes from two distinct sources are of the same pitch, their rates of vibration are the same. If, for example, a string yield the same note as a tuning-fork, it is because they vibrate with the same rapidity; and if a

fork yield the same note as the pipe of an organ or the tongue of a concertina, it is because the vibrations of the fork in the one case are executed in precisely the same time as the vibrations of the column of air, or of the tongue, in the other. The same holds good for the human voice. If a string and a voice yield the same note, it is because the vocal chords of the singer vibrate in the same time as the string vibrates. Is there any way of determining the actual number of vibrations corresponding to a musical note? Can we infer from the pitch of a string, of an organ-pipe, of a tuning-fork, or of the human voice, the number of waves which it sends forth in a second? This very beautiful problem is capable of the most complete solution.

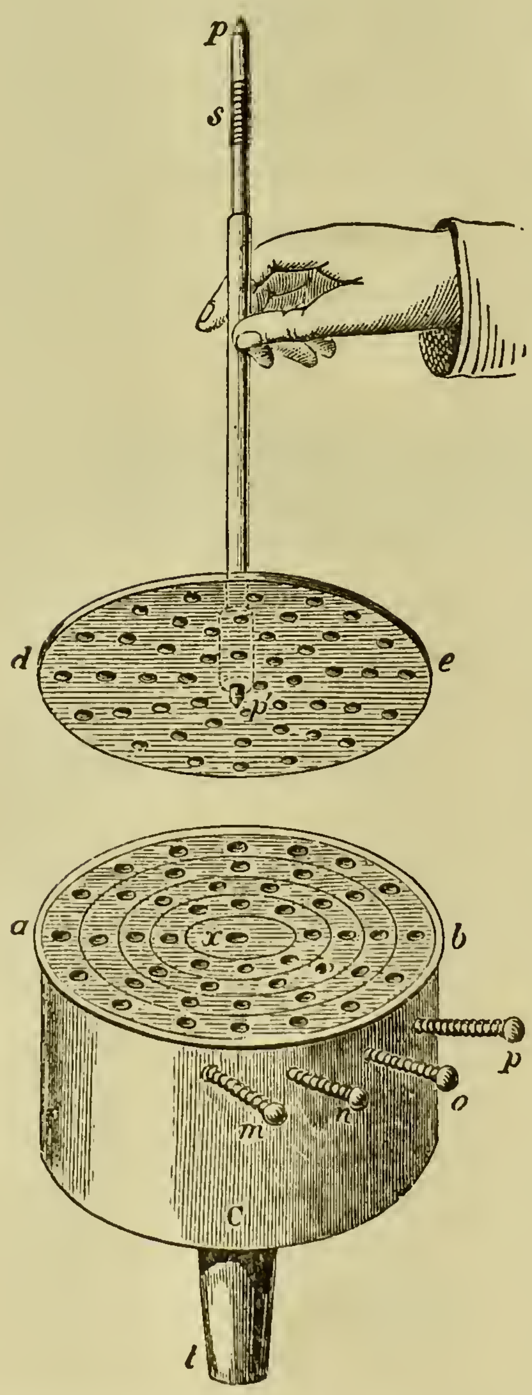
§ 7. *The Syren: analysis of the Instrument.*

By the rotation of a perforated pasteboard disc, it has been just proved that a musical sound is produced by a quick succession of puffs. Had we any means of registering the number of revolutions accomplished by that disc in a minute, we should have in it a means of determining the number of puffs per minute due to a note of any determinate pitch. The disc, however, is but a cheap substitute for a far more perfect apparatus, which requires no whirling table, and which registers its own rotations with the most perfect accuracy.

I will take the instrument asunder, so that you may see its various parts. A brass tube *t*, fig. 26, leads into a round box C, closed at the top by a brass plate *a b*. This plate is perforated with four series of holes, placed along four concentric circles. The innermost series contains 8, the next 10, the next 12, and the outermost 16 orifices. When we blow into the tube *t*, the air escapes through the orifices, and the problem now before us is to convert these continuous currents into discontinuous puffs.

This is accomplished by means of a brass disc *d e*, also perforated with 8, 10, 12, and 16 holes, at the same distances from the centre and with the same intervals between them as those in the top of the box C. Through the centre of the disc passes a steel axis, the two ends of which are smoothly bevelled off to points at *p* and *p′*. My object now is to cause this perforated disc to rotate over the perforated top *a b* of the box C. You will understand how this is done by observing how the instrument is put together.

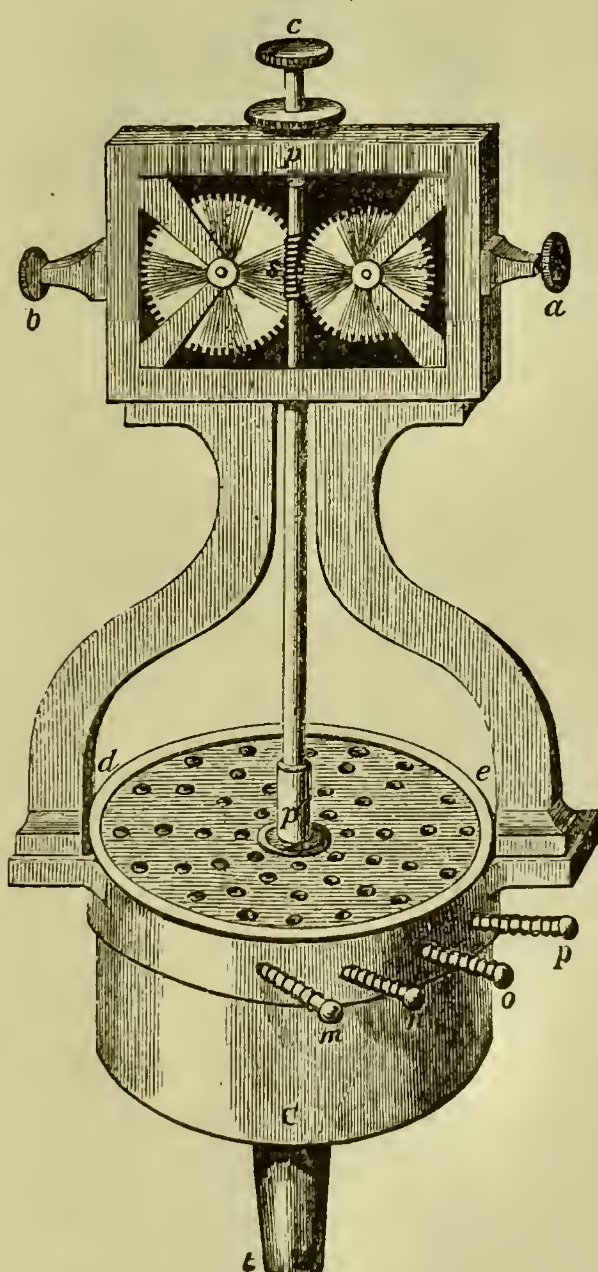
Fig. 26.



In the centre of *a b*, fig. 26, is a depression *x* sunk in steel, smoothly polished and intended to receive the end *p′* of the axis. I place the end *p′* in this depression, and, holding the axis upright, bring down upon its upper end *p* a steel cap, finely polished within, which holds the axis at the top, the pressure both at top and bottom being so gentle, and the polish of the touching surfaces so perfect, that the disc can rotate with an exceedingly small amount of friction. At *c*, fig. 27, is the cap which fits on to the upper end of the axis *p p′*. In this figure the disc *d e* is shown covering the top of the cylinder C. You may neglect for the present the wheel-

work of the figure. Turning the disc *d e* slowly round, its perforations may be caused to coincide or not coincide with those of the cylinder underneath. As the disc turns its orifices come alternately over the perforations of the cylinder, and over the spaces between the perforations. Hence it is plain that if air were urged into C, and if the disc could be caused to rotate at the same time, we should accomplish our object, and carve into puffs the streams of air. In this beautiful instrument the disc is caused to rotate by the very air currents which it renders intermittent. This is done by the simple device of causing the perforations to pass *obliquely* through the top of the cylinder C, and to impinge on the oppositely inclined orifices of the rotating disc *d e*. The air is thus caused to issue from C, not vertically, but in side currents, which impinge against the disc and drive it round. In this way, by its passage through the syren. the air is moulded into sonorous waves.

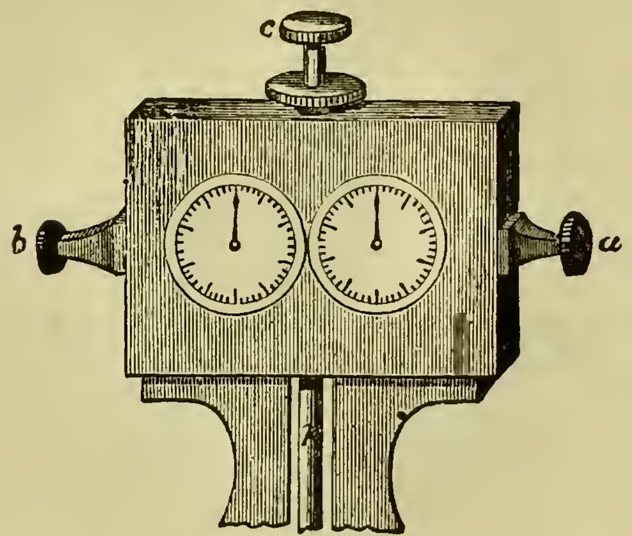
FIG. 27.



Another moment will make you acquainted with the recording portion of the instrument. At the upper part of the steel axis p p', fig. 27, is a screw s, working into a

pair of toothed wheels (seen when the back of the instrument is turned towards you). As the disc and its axis turn, these wheels rotate. In front you simply see two graduated dials, fig. 28, each furnished with an index like the hand of a clock. These indexes record the number of revolutions executed by the disc in any given time. By pushing the button *a* or *b*, the wheelwork is thrown into or out of action, thus starting or suspending, in a moment, the process of recording. Finally, by the pins *m*, *n*, *o*, *p*, fig 27, any series of orifices in the top of the cylinder C can be opened or closed at pleasure. By pressing *m*, one series is opened; by pressing *n*, another. By pressing two keys, two series of orifices are opened; by pressing three keys, three series; and by pressing all the keys, puffs are caused to issue from the four series simultaneously. The perfect instrument is now before you, and your knowledge of it is complete.

Fig. 28.



This instrument received the name of syren from its inventor, Cagniard de la Tour. The one now before you is the syren as greatly improved by Dove. The pasteboard syren, whose performance you have already heard, was devised by Seebeck, who gave the instrument various interesting forms, and executed with it many important experiments. Let us now make the syren sing. By pressing the key *m*, the outer series of apertures in the cylinder C is opened, and by working the bellows, the air is caused to impinge against the disc. It begins to rotate, and you hear a succession of puffs which follow each

other so slowly that they may be counted. But as the motion augments, the puffs succeed each other with increasing rapidity, and at length you hear a deep musical note. As the velocity of rotation increases the note rises in pitch: it is now very clear and full, and as the air is urged more vigorously, it becomes so shrill as to be painful. Here we have a further illustration of the dependence of pitch on rapidity of vibration. I touch the side of the disc and lower its speed; the pitch falls instantly. Continuing the contact the tone continues to sink, ending in the discontinuous puffs with which it began.

Were the blast sufficiently powerful and the syren sufficiently free from friction, it might be urged to higher and higher notes, until finally its sound would become inaudible to human ears. This, however, would not prove the absence of vibratory motion in the air; but would rather show that our auditory apparatus is incompetent to take up and translate into sound vibrations whose rapidity exceeds a certain limit. The ear, as we shall immediately learn, is in this respect similar to the eye.

By means of the syren we can determine with extreme accuracy the rapidity of vibration of any sonorous body. It may be a vibrating string, an organ pipe, a reed, or the human voice. Operating delicately, we might even determine from the hum of an insect the number of times it flaps its wings in a second. I will illustrate the subject by determining in your presence a tuning-fork's rapidity of vibration. From the acoustic bellows I urge the air through the syren, and, at the same time, draw my bow across the fork. Both now sound together, the tuning-fork yielding at present the highest note. But the pitch of the syren gradually rises, and at length you hear the 'beats' so well known to musicians, which indicate that the two notes are not wide apart in pitch. These beats become slower and slower; now they entirely

vanish, both notes blending as it were to a single stream of sound.

All this time the clockwork of the syren has remained out of action. As the second hand of a watch crosses the number 60, the clockwork is set going by pushing the button *a*. We will allow the disc to continue its rotation for a minute, the tuning-fork being excited from time to time to assure you that the unison is preserved. The second hand again approaches 60; as it passes that number the clockwork is stopped by pushing the button *b*; and then, recorded on the dials, we have the exact number of revolutions performed by the disc. The number is 1,440. But the series of holes open during the experiment numbers 16; for every revolution, therefore, we had 16 puffs of air, or 16 waves of sound. Multiplying 1,440 by 16, we obtain 23,040 as the number of vibrations executed by the tuning-fork in a minute. Dividing this by 60, we find the number of vibrations executed in a second to be 384.

§ 8. *Determination of Wave-lengths : time of Vibration.*

Having determined the rapidity of vibration, the length of the corresponding sonorous wave is found with the utmost facility. Imagine a tuning-fork vibrating in free air. At the end of a second from the time it commenced its vibrations the foremost wave would have reached a distance of 1,090 feet in air of the freezing temperature. In the air of a room which has a temperature of about 15° C., it would reach a distance of 1,120 feet in a second. In this distance, therefore, are embraced 384 sonorous waves. Dividing 1,120 by 384, we find the length of each wave to be nearly 3 feet. Determining with the syren the rates of vibration of the four tuning-forks now before you, we find them to be 256, 320, 384, and

512; these numbers corresponding to wave-lengths of 4 feet 4 inches, 3 feet 6 inches, 2 feet 11 inches, and 2 feet 2 inches respectively. The waves generated by a man's voice in common conversation are from 8 to 12 feet, those of a woman's voice are from 2 to 4 feet in length. Hence a woman's ordinary pitch in the lower sounds of conversation is more than an octave above a man's; in the higher sounds it is two octaves.

And here it is important to note that by the term vibrations are meant *complete ones*; and by the term sonorous wave are meant a condensation and its associated rarefaction. By a vibration an excursion *to and fro* of the vibrating body is to be understood. Every wave generated by such a vibration bends the tympanic membrane once in and once out. These are the definitions of a vibration and of a sonorous wave employed in England and Germany. In France, however, a vibration consists of an excursion of the vibrating body *in one direction*, whether to or fro. The French vibrations, therefore, are only the halves of ours, and we therefore call them semi-vibrations. In all cases throughout these chapters, when the word vibration is employed without qualification, it refers to complete vibrations.

During the time required by each of those sonorous waves to pass entirely over a particle of air, that particle accomplishes one complete vibration. It is at one moment pushed forward into the condensation, while at the next moment it is urged back into the rarefaction. The time required by the particle to execute a complete oscillation is, therefore, that required by the sonorous wave *to move through a distance equal to its own length.* Supposing the length of the wave to be 8 feet, and the velocity of sound in air of our present temperature to be 1,120 feet a second, the wave in question will pass over its own length of air in $\frac{1}{140}$th of a second: this is the time required by every

air-particle which the wave passes in its course to complete an oscillation.

In air of a definite density and elasticity a certain length of wave always corresponds to the same pitch. But supposing the density or elasticity not to be uniform; supposing, for example, the sonorous waves from one of our tuning-forks to pass from cold to hot air, an instant augmentation of the wave-length would occur, without any change of pitch, for we should have no change in the rapidity with which the waves would reach the ear. Conversely with the same length of wave the pitch would be higher in hot air than in cold, for the succession of the waves would be quicker. In an atmosphere of hydrogen waves of a certain length would produce a note two octaves higher than waves of the same length in air; for, in consequence of the greater rapidity of propagation, the number of impulses received in a given time in the one case would be four times the number received in the other.

§ 9. *Definition of an Octave.*

Opening the innermost and outermost series of the orifices of our syren, and sounding both of them, either together or in succession, the musical ears present at once detect the relationship of the two sounds. They notice immediately that the sound which issues from the circle of 16 orifices is the octave of that which issues from the circle of 8. But for every wave sent forth by the latter, two waves are sent forth by the former. In this way we prove the physical meaning of the term 'octave' to be, a note produced by double the number of vibrations of its fundamental. By multiplying the vibrations of the octave by two, we obtain *its* octave, and by a continued multiplication of this kind we obtain a series of numbers answering to a series of octaves. Starting, for example

from a fundamental note of 100 vibrations, we should find by this continual multiplication that a note five octaves above it would be produced by 3,200 vibrations. Thus:—

100	Fundamental note
2	
200	1st octave
2	
400	2nd octave
2	
800	3rd octave
2	
1600	4th octave
2	
3200	5th octave

This result is more readily obtained by multiplying the vibrations of the fundamental note by the fifth power of two. In a subsequent lecture we shall return to this question of musical intervals. For our present purpose it is only necessary to define an octave.

§ 10. *Limits of the Ear; and of Musical Sounds.*

The ear's range of hearing is limited in both directions. Savart fixed the lower limit at eight complete vibrations a second; and to cause these slowly recurring vibrations to link themselves together, he was obliged to employ shocks of great power. By means of a toothed wheel and an associated counter, he fixed the upper limit of hearing at 24,000 vibrations a second. Helmholtz has recently fixed the lower limit at 16 vibrations, and the higher at 38,000 vibrations, a second. By employing very small tuning-forks, the late M. Depretz showed that a sound corresponding to 38,000 vibrations a second is audible.[1] Starting

[1] The error of Savart consists, according to Helmholtz, in having adopted an arrangement in which overtones (described in Lecture III.) were mistaken for the fundamental one.

from the note 16 and multiplying continually by 2, or more compendiously raising 2 to the 11th power, and multiplying this by 16, we should find that at 11 octaves above the fundamental note the number of vibrations would be 32,768. Taking, therefore, the limits assigned by Helmholtz, the entire range of the human ear embraces about eleven octaves. But all the notes comprised within these limits cannot be employed in music. The practical range of musical sounds is comprised between 40 and 4,000 vibrations a second, which amounts, in round numbers, to 7 octaves.[1]

The limits of hearing are different in different persons. While endeavouring to estimate the pitch of certain sharp sounds, Dr. Wollaston remarked in a friend a total insensibility to the sound of a small organ-pipe, which, in respect to acuteness, was far within the ordinary limits of hearing. The sense of hearing of this person terminated at a note four octaves above the middle E of the pianoforte. The squeak of a bat, the sound of a cricket, even the chirrup of the common house-sparrow, are unheard by some people who for lower sounds possess a sensitive ear. A difference of a single note is sometimes sufficient to produce the change from sound to silence. 'The suddenness of the transition,' writes Wollaston, 'from perfect hearing to total

[1] 'The deepest tone of orchestra instruments is the E of the double-bass, with $41\frac{1}{4}$ vibrations. The new pianos and organs go generally as far as C^{I} with 33 vibrations; new grand pianos may reach A^{II} with $27\frac{1}{2}$ vibrations. In large organs a lower octave is introduced reaching to C^{II} with $16\frac{1}{2}$ vibrations. But the musical character of all these tones under E is imperfect, because they are near the limit where the power of the ear to unite the vibrations to a tone ceases. In height the pianoforte reaches to a^{IV} with 3,520 vibrations, or sometimes to c^{V} with 4,224 vibrations. The highest note of the orchestra is probably the d^{V} of the piccolo flute, with 4,752 vibrations.'—Helmholtz, *Tonempfindungen*, p. 30. In this notation we start from C with 66 vibrations, calling the first lower octave C^{I}, and the second C^{II}; and calling the first highest octave c, the second c^{I}, the third c^{II}, the fourth c^{III}, &c. In England the deepest tone, Mr. Macfarren informs me, is not E but A, a fourth above it.

want of perception, occasions a degree of surprise which renders an experiment of this kind with a series of small pipes among several persons rather amusing. It is curious to observe the change of feeling manifested by various individuals of the party, in succession, as the sounds approach and pass the limits of their hearing. Those who enjoy a temporary triumph are often compelled, in their turn, to acknowledge to how short a distance their little superiority extends.' 'Nothing can be more surprising,' writes Sir John Herschel, 'than to see two persons, neither of them deaf, the one complaining of the penetrating shrillness of a sound, while the other maintains there is no sound at all. Thus, while one person mentioned by Dr. Wollaston could but just hear a note 4 octaves above the middle E of the pianoforte, others have a distinct perception of sounds full 2 octaves higher. The chirrup of the sparrow is about the former limit; the cry of the bat about an octave above it; and that of some insects probably another octave.' In 'The Glaciers of the Alps' I have referred to a case of short auditory range noticed by myself, in crossing the Wengern Alp in company with a friend. The grass at each side of the path swarmed with insects, which to me rent the air with their shrill chirruping. My friend heard nothing of this, the insect-music lying beyond his limit of audition.[1]

§ 11. *Drum of the Ear. The Eustachian Tube.*

Behind the tympanic membrane exists a cavity—the drum of the ear—in part crossed by a series of bones, and in part occupied by air. This cavity communicates with the mouth by means of a duct called the Eustachian tube. This tube is generally closed, the air-space behind the tym-

[1] By means of small pipes devised by himself, the higher limit of hearing has been recently investigated with characteristic genius by Mr. Francis Galton.

panic membrane being thus shut off from the external air. If, under these circumstances, the external air becomes denser, it will press the tympanic membrane inwards. If, on the other hand, the air outside become rarer, while the Eustachian tube remains closed, the membrane will be pressed outwards. Pain is felt in both cases, and partial deafness is experienced. I once crossed the Stelvio Pass by night in company with a friend who complained of acute pain in the ears. On swallowing his saliva the pain instantly disappeared. By the act of swallowing the Eustachian tube is opened, and thus equilibrium is established between the external and internal pressure.

It is possible to quench the sense of hearing of low sounds by stopping the nose and mouth, and trying to expand the chest, as in the act of inspiration. This effort partially exhausts the space behind the tympanic membrane, which is then thrown into a state of tension by the pressure of the outward air. A similar deafness to low sounds is produced when the nose and mouth are stopped, and a strong effort is made to expire. In this case air is forced through the Eustachian tube into the drum of the ear, the tympanic membrane being distended by the pressure of the internal air. The experiment may be made in a railway carriage, when the low rumble will vanish or be greatly enfeebled, while the sharper sounds are heard with undiminished intensity. Dr. Wollaston was expert in closing the Eustachian tube, and leaving the space behind the tympanic membrane occupied by either compressed or rarefied air. He was thus able to cause his deafness to continue for any required time without effort on his part, always, however, abolishing it by the act of swallowing. A sudden concussion may produce deafness by forcing air either into or out of the drum of the ear, and this *may* account for a fact noticed by myself in one of my Alpine rambles. In the summer of 1858, jumping from a cliff

on to what was supposed to be a deep snow-drift, I came into rude collision with a rock which the snow barely covered. The sound of the wind, the rush of the glacier-torrents, and all the other noises which a sunny day awakes upon the mountains, instantly ceased. I could hardly hear the sound of my guide's voice. This deafness continued for half-an-hour; at the end of which time the blowing of the nose opened, I suppose, the Eustachian tube, and restored, with the quickness of magic, the innumerable murmurs which filled the air around me.

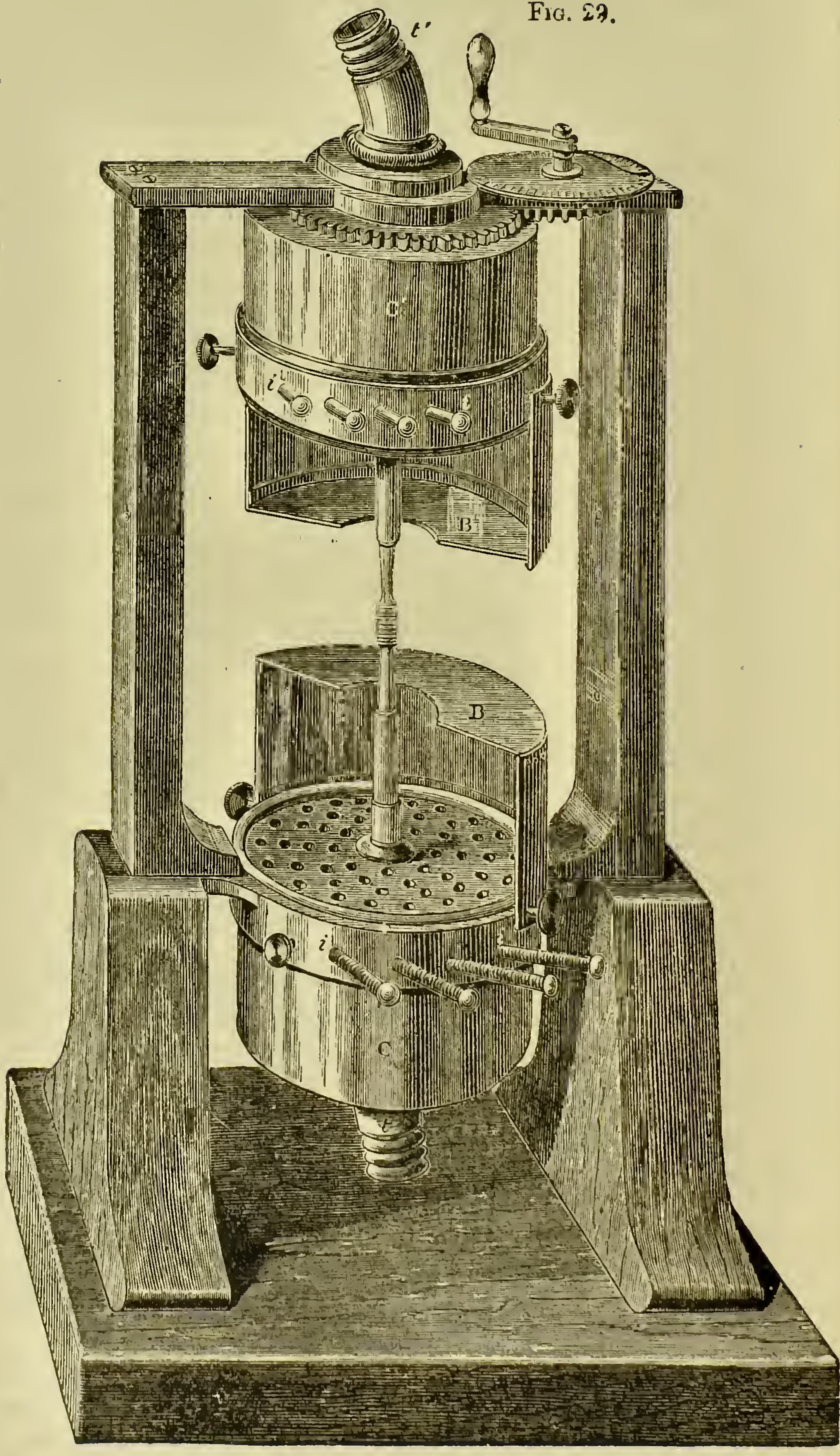
Light, like sound, is excited by pulses or waves; and lights of different colours, like sounds of different pitch, are excited by different rates of vibration. But in its width of perception the ear far transcends the eye; for while the former ranges over 11 octaves, but little more than a single octave is possible to the latter. The quickest vibrations which strike the eye, as light, have only about twice the rapidity of the slowest;[1] whereas the quickest vibrations which strike the ear, as a musical sound, have more than two thousand times the rapidity of the slowest.

§ 12. *Helmholtz's Double Syren.*

Professor Dove, as we have seen, extended the utility of the syren of Cagniard de la Tour, by providing it with four series of orifices instead of one. By doubling all its parts, Helmholtz has recently added vastly to the power of the instrument. The double syren, as it is called, is now before you, fig. 29 (next page). It is composed of two of Dove's syrens, C and C′, one turned upside down. You will recognise in the lower syren the instrument with which you are already acquainted. The discs of the two

[1] It is hardly necessary to remark that the quickest vibrations and shortest waves correspond to the extreme violet, while the slowest vibrations and longest waves correspond to the extreme red, of the spectrum.

FIG. 29.



syrens have a common axis, so that when one disc rotates the other rotates with it. As in the former case, the number of revolutions is recorded by clockwork (omitted in the figure). When air is urged through the tube t' the upper syren alone sounds; when urged through t, the lower one only sounds; when it is urged simultaneously through t' and t, both the syrens sound. With this instrument, therefore, we are able to introduce much more varied combinations than with the former one. Helmholtz has also contrived a means by which not only the disc of the upper syren, but the box C′ above the disc, can be caused to rotate. This is effected by a toothed wheel and pinion, turned by a handle. Underneath the handle is a dial with an index, the use of which will be subsequently illustrated.

Let us direct our attention for the present to the upper syren. By means of an india-rubber tube, the orifice t' is connected with an acoustic bellows, and air is urged into C′. Its disc turns round, and we obtain with it all the results already obtained with Dove's syren. The pitch of the note is uniform. Turning the handle above, so as to cause the orifices of the cylinder C′ to *meet* those of the disc, the two sets of apertures pass each other more rapidly than when the cylinder stood still. An instant rise of pitch is the result. By reversing the motion, the orifices are caused to pass each other more slowly than when C′ is motionless, and in this case you notice an instant fall of pitch when the handle is turned. Thus, by imparting in quick alternation a right-handed and left-handed motion to the handle, we obtain successive rises and falls of pitch. An extremely instructive effect of this kind may be observed at any railway station on the passage of a rapid train. During its approach the sonorous waves emitted by the whistle are virtually shortened, a greater number of them being crowded into the ear in a given time.

During its retreat we have a virtual lengthening of the sonorous waves. The consequence is that, when approaching, the whistle sounds a higher note, and when retreating it sounds a lower note, than if the train were still. A fall of pitch, therefore, is perceived as the train passes the station.[1] This is the basis of Doppler's theory of the coloured stars. He supposes that all stars are white, but that some of them are rapidly retreating from us, thereby lengthening their luminiferous waves and becoming red. Others are rapidly approaching us, thereby shortening their waves, and becoming green and blue. The ingenuity of this theory is extreme, but its correctness is more than doubtful.

§ 13. *Transmission of Musical Sounds by Liquids and Solids.*

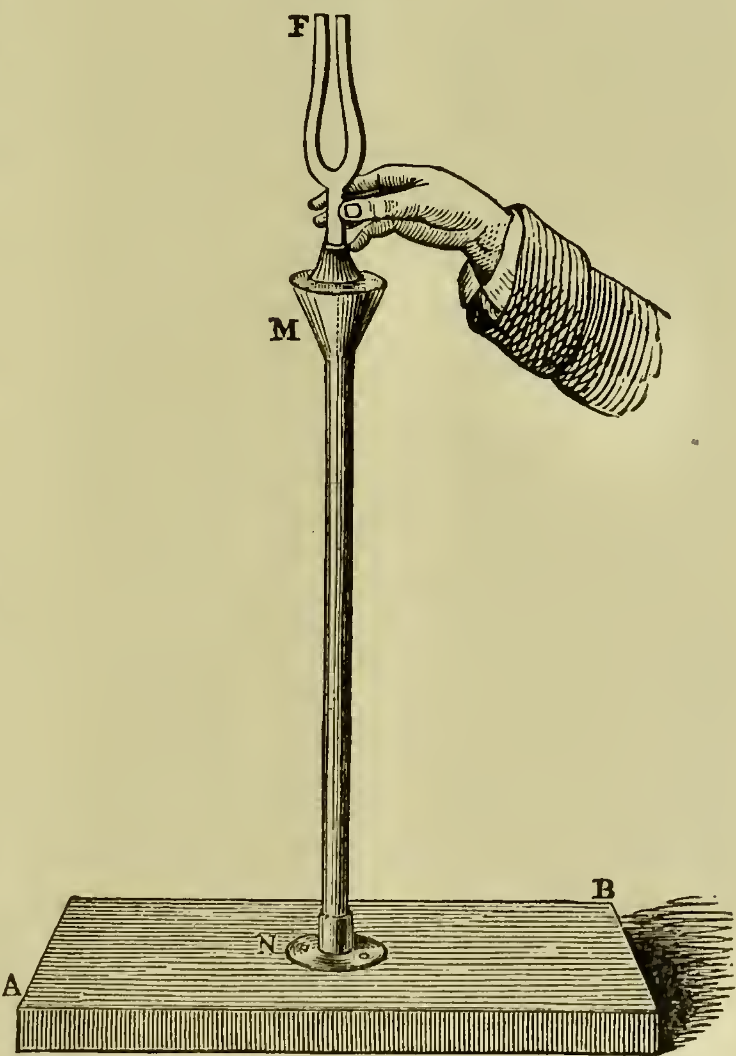
We have thus far occupied ourselves with the transmission of musical sounds through air. They are also transmitted by liquids and solids. When a tuning-fork screwed into a little wooden foot vibrates in free air, only the persons closest to it hears it sound. On dipping the foot into a glass of water a musical sound is audible: the vibrations having been transmitted through the water to the air. The tube M N, fig. 30, three feet long, is set upright upon a wooden tray A B. The tube ends in a funnel at the top, and is now filled with water to the brim. The fork F is thrown into vibration, and on dipping its foot into the funnel M, a musical sound swells out. I must so far forestall matters as to remark, that in this experiment the tray is the real sounding body. It has been thrown into vibration by the fork,

[1] Experiments on this subject were first made by M. Buys Ballot on the Dutch railway, and subsequently by Mr. Scott Russell in this country. Doppler's idea is now applied to determine, from changes of wave-length, motions in the sun and fixed stars.

but the vibrations have been conveyed to the tray *by the water.* Through the same medium vibrations are communicated to the auditory nerve, the terminal filaments of which are immersed in a liquid. Substituting mercury for water in this experiment, a similar result is obtained.

The syren has received its name from its capacity to sing under water. A vessel now in front of the table is half filled with the liquid, in which a syren is wholly immersed. When a cock is turned the water from the pipes which supply the house forces itself through the instrument. Its disc is now rotating, and a sound of rapidly augmenting pitch issues from the vessel. The pitch rises thus rapidly because the heavy and powerfully pressed water soon drives the disc up to its maximum speed of rotation When the supply is lessened, the motion relaxes and the pitch falls. Thus, by alternately opening and closing the cock, the song of the syren is caused to rise and fall in a wild and melancholy manner. You would not consider such a sound likely to woo mariners to their doom.

Fig. 30.



The transmission of musical sounds through solid bodies is also capable of easy and agreeable illustration. Before you is a wooden rod, thirty feet long, passing from the table through a window in the ceiling, into the open air above. The lower end of the rod rests upon a wooden tray, to which the musical vibrations of a body applied to the upper end of the rod are to be transferred. An assistant is above, with a tuning-fork in his hand. He strikes the fork against a pad; it vibrates, but you hear nothing. He now applies the stem of the fork to the end of the rod, and instantly the wooden tray upon the table is rendered musical. The pitch of the sound, moreover, is exactly that of the tuning-fork; the wood has been passive as regards pitch, transmitting the precise vibrations imparted to it, without any alteration. With another fork a note of another pitch is obtained. Thus fifty forks might be employed instead of two, and 300 feet of wood instead of 30; the rod would transmit the precise vibrations imparted to it, and no others.

We are now prepared to appreciate an extremely beautiful experiment, for which we are indebted to Sir Charles Wheatstone. In a room underneath this, and separated from it by two floors, is a piano. Through the two floors passes a tin tube $2\frac{1}{2}$ inches in diameter, and along the axis of this tube passes a rod of deal, the end of which emerges from the floor in front of the lecture table. The rod is clasped by india-rubber bands, which entirely close the tin tube. The lower end of the rod rests upon the sound-board of the piano, its upper end being exposed before you. An artist is at this moment engaged at the instrument, but you hear no sound. When, however, a violin is placed upon the end of the rod, the instrument becomes instantly musical, not, however, with the vibrations of its own strings, but with those of the piano. When the violin is removed, the sound ceases; putting in

its place a guitar, the music revives. For the violin and guitar we may substitute a plain wooden tray, which is also rendered musical. Here, finally, is a harp, against the sound-board of which the end of the deal rod is caused to press; every note of the piano is reproduced before you. On lifting the harp so as to break the connection with the piano, the sound vanishes; but the moment the sound-board is caused to press upon the rod the music is restored. The sound of the piano so far resembles that of the harp that it is hard to resist the impression that the music you hear is that of the latter instrument. An uneducated person might well believe that witchcraft or 'spiritualism' is concerned in the production of this music.

What a curious transference of action is here presented to the mind! At the command of the musician's will, the fingers strike the keys; the hammers strike the strings, by which the rude mechanical shock is converted into tremors. The vibrations are communicated to the sound-board of the piano. Upon that board rests the end of the deal rod, thinned off to a sharp edge to make it fit more easily between the wires. Through the edge, and afterwards along the rod, are poured with unfailing precision the entangled pulsations produced by the shocks of those ten agile fingers. To the sound-board of the harp before you the rod faithfully delivers up the vibrations of which it is the vehicle. This second sound-board transfers the motion to the air, carving it and chasing it into forms so transcendently complicated that confusion alone could be anticipated from the shock and jostle of the sonorous waves. But the marvellous human ear accepts every feature of the motion, and all the strife and struggle and confusion melt finally into music upon the brain.[1]

[1] An ordinary musical box may be substituted for the piano in this experiment.

SUMMARY OF LECTURE II.

A musical sound is produced by pulses or waves which follow each other at regular intervals with sufficient rapidity of succession.

Noise is produced by an irregular succession of sonorous pulses.

A musical sound may be produced by *taps* which rapidly and regularly succeed each other. The taps of a card against the cogs of a rotating wheel are usually employed to illustrate this point.

A musical sound may also be produced by a succession of *puffs*. The syren is an instrument by which such puffs are generated.

The pitch of a musical note depends solely on the number of vibrations concerned in its production. The more rapid the vibrations, the higher the pitch.

By means of the syren the rate of vibration of any sounding body may be determined. It is only necessary to render the note of the syren and that of the body identical in pitch, to maintain both sounds in unison for a certain time, and to ascertain, by means of the counter of the syren, how many puffs have issued from the instrument in that time. This number expresses the number of vibrations executed by the sounding body.

When a body capable of emitting a musical sound—a tuning-fork, for example—vibrates, it moulds the surrounding air into sonorous waves, each of which consists of a condensation and a rarefaction.

The length of the sonorous wave is measured from

condensation to condensation, or from rarefaction to rarefaction.

The wave-length is found by dividing the velocity of sound per second by the number of vibrations executed by the sounding body in a second.

Thus a tuning-fork which vibrates 256 times in a second produces in air of 15° C., where the velocity is 1,120 feet a second, waves 4 feet 4 inches long. While two other forks, vibrating respectively 320 and 384 times a second, generate waves 3 feet 6 inches and 2 feet 11 inches long.

A vibration, as defined in England and Germany, comprises a motion to *and* fro. It is a *complete* vibration. In France, on the contrary, a vibration comprises a movement to *or* fro. The French vibrations are with us semi-vibrations.

The time required by a particle of air over which a sonorous wave passes to execute a complete vibration is that required by the wave to move through a distance equal to its own length.

The higher the temperature of the air, the longer is the sonorous wave corresponding to any particular rate of vibration. Given the wave-length and the rate of vibration, we can readily deduce the temperature of the air.

The human ear is limited in its range of hearing musical sounds. If the vibrations number less than 16 a second, we are conscious only of the separate shocks. If they exceed 38,000 a second, the consciousness of sound ceases altogether. The range of the best ear covers about 11 octaves, but an auditory range limited to 6 or 7 octaves is not uncommon.

The sounds available in music are produced by vibrations comprised between the limits of 40 and 4,000 a second. They embrace 7 octaves.

The range of the ear far transcends that of the eye, which hardly exceeds an octave.

By means of the Eustachian tube, which is opened in the act of swallowing, the pressure of the air on both sides of the tympanic membrane is equalised.

By either condensing or rarefying the air behind the tympanic membrane, deafness to sounds of low pitch may be produced.

On the approach of a railway train the pitch of the whistle is higher, on the retreat of the train the pitch is lower, than it would be if the train were at rest.

Musical sounds are transmitted by liquids and solids. Such sounds may be transferred from one room to another; from the ground-floor to the garret of a house of many stories, for example, the sound being unheard in the rooms intervening between both, and rendered audible only when the vibrations are communicated to a suitable sound-board

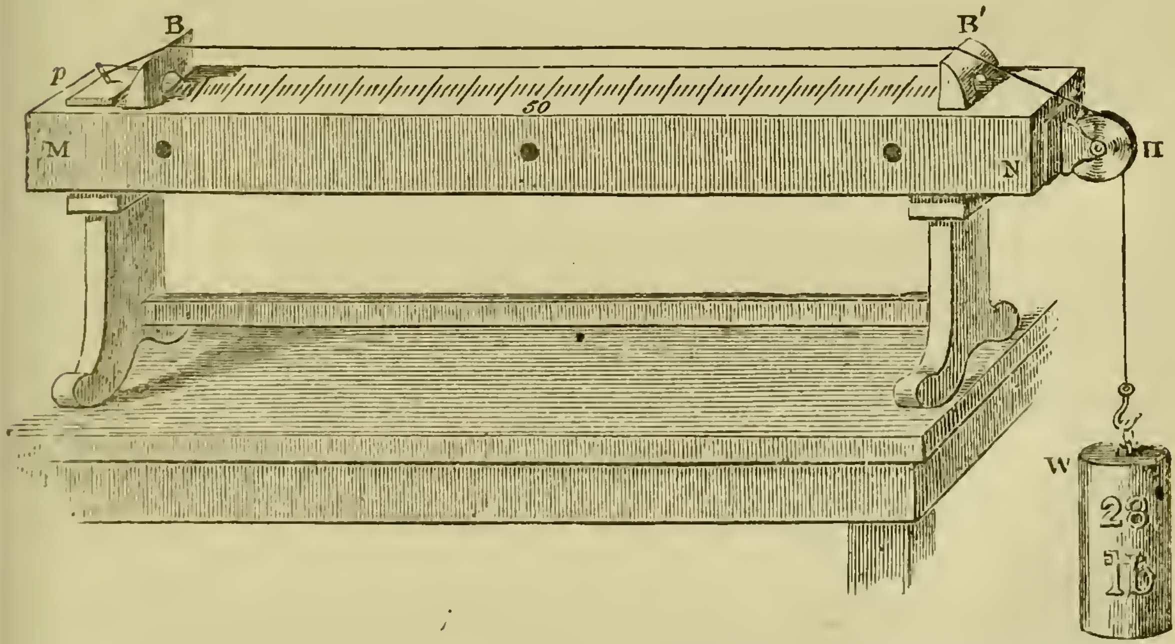
LECTURE III.

VIBRATION OF STRINGS—INFLUENCE OF SOUND-BOARDS—LAWS OF VIBRATING STRINGS—STATIONARY AND PROGRESSIVE WAVES—NODES AND VENTRAL SEGMENTS—APPLICATION OF RESULTS TO THE VIBRATIONS OF MUSICAL STRINGS—EXPERIMENTS OF MELDE—STRINGS SET IN VIBRATION BY TUNING-FORKS—LAWS OF VIBRATION THUS DEMONSTRATED—HARMONIC TONES OF STRINGS—DEFINITION OF TIMBRE OR QUALITY, OF OVERTONES AND CLANG—ABOLITION OF SPECIAL HARMONICS—CONDITIONS WHICH AFFECT THE INTENSITY OF THE HARMONIC TONES—OPTICAL EXAMINATION OF THE VIBRATIONS OF A PIANO-WIRE.

§ 1. *Vibrations of Strings: use of Sound-boards.*

WE have to begin our studies to-day with the vibrations of strings or wires; to learn how bodies of this

FIG. 31.



form are rendered available as sources of musical sounds, and to investigate the laws of their vibrations.

To enable a musical string to vibrate *transversely*, or at right angles to its length, it must be stretched between

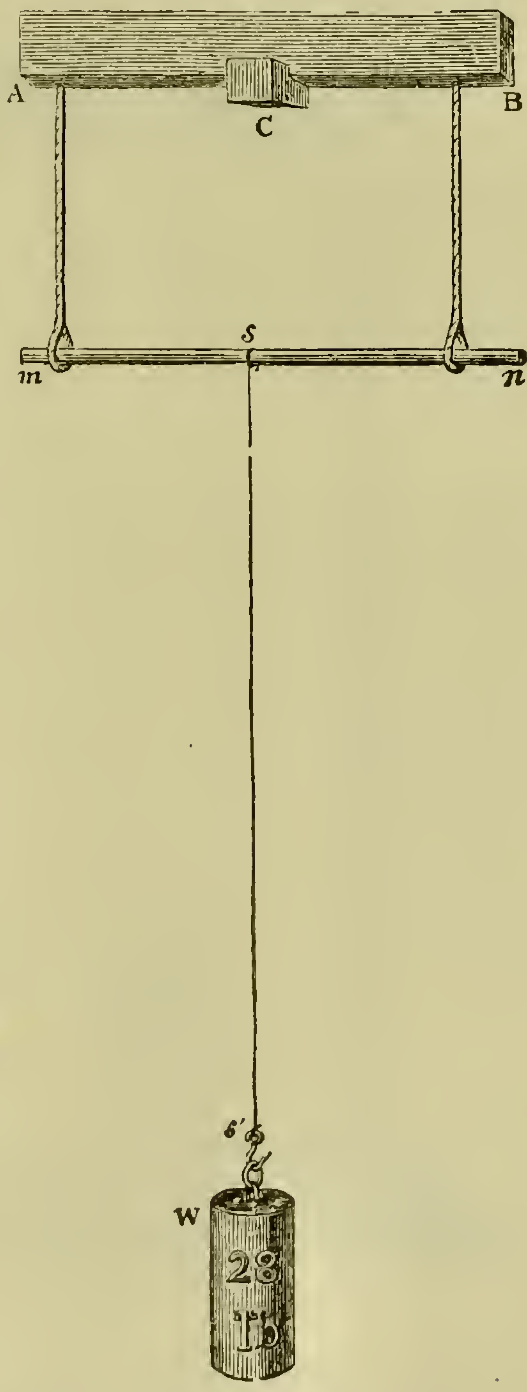
two rigid points. Before you, fig. 31, is an instrument employed to stretch strings, and to render their vibrations audible. From the pin *p*, to which one end of it is firmly attached, a string passes across the two bridges B and B′, being afterwards carried over the wheel H, which moves with great freedom. The string is finally stretched by a weight W of 28 lbs. attached to its extremity. The bridges B and B′, which constitute the real ends of the string, are fastened on to the long wooden box M N. The whole instrument is called a monochord or sonometer.

Taking hold of the stretched string B B′ at its middle and plucking it aside, it springs back to its first position, passes it, returns, and thus vibrates for a time to and fro across its position of equilibrium. You hear a sound, but the sonorous waves which at present strike your ears do not proceed immediately from the string. The amount of wave-motion generated by so thin a body is too small to be sensible at any distance. But the string is drawn tightly over the two bridges B B′; and when it vibrates, its tremors are communicated through these bridges to the entire mass of the box M N, and to the air within the box, which thus become the real sounding bodies.

That the vibrations of the string alone are not sufficient to produce the sound may be thus experimentally demonstrated:—A B, fig. 32, is a piece of wood placed across an iron bracket C. From each end of the piece of wood depends a rope ending in a loop, while stretching across from loop to loop is an iron bar *m n*. From the middle of the iron bar hangs a steel wire *s s′*, stretched by a weight W of 28 lbs. By this arrangement the wire is detached from all large surfaces to which it could impart its vibrations. Plucking the wire *s s′* in the middle, it vibrates vigorously, but even those nearest to it do not hear any sound. The agitation imparted to the air is too inconsiderable to affect the auditory nerve. A second

wire $t\ t'$, fig. 33 (next page), of the same length, thickness, and material as $s\ s'$, has one of its ends attached to the wooden tray A B. This wire also carries a weight W of 28 lbs. Finally, passing over the bridges B B′ of the sonometer, fig. 31, is our third wire, in every respect like the two others, and like them stretched by a weight W of 28 lbs. When the wire $t\ t'$, fig. 33, is caused to vibrate, you hear its sound distinctly. Though one end only of the wire is connected with the tray, A B, the vibrations transmitted to it are sufficient to convert the tray into a sounding body. Finally, when the wire of the sonometer M N, fig. 31, is plucked, the sound is loud and full, because the instrument is specially constructed to take up the vibrations of the wire.

FIG. 32.

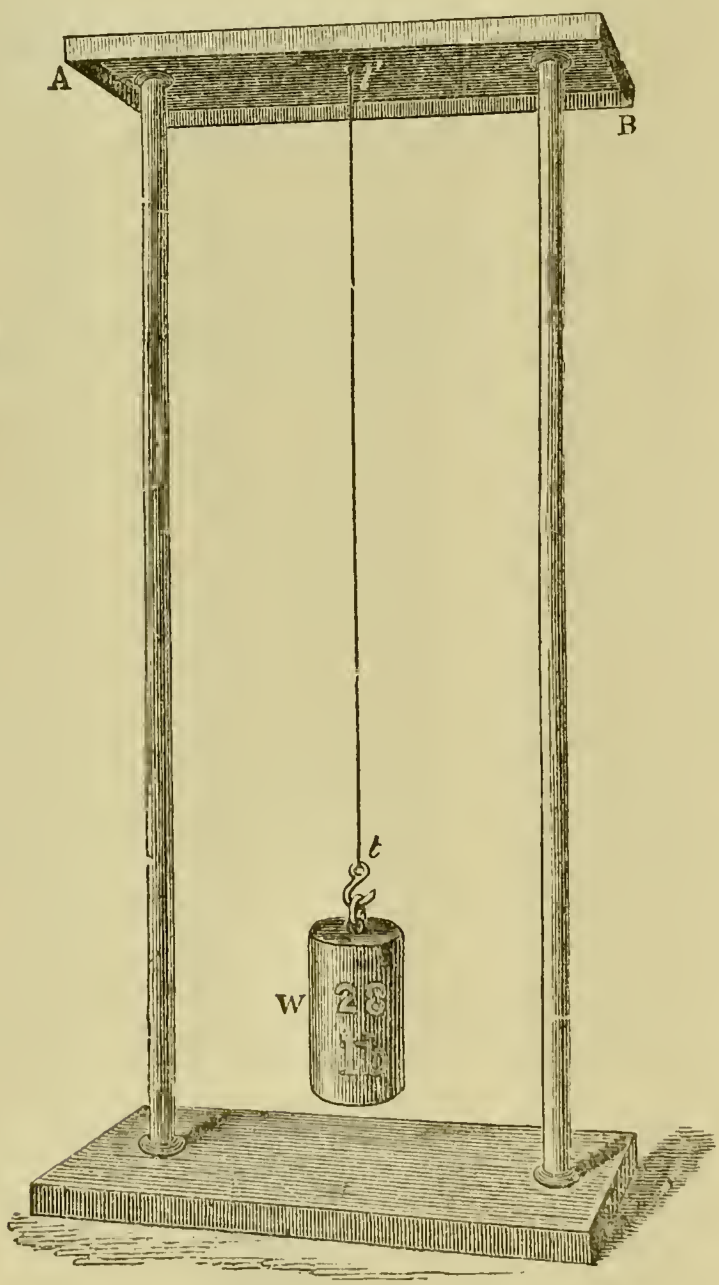


The importance of employing proper sounding apparatus in stringed instruments is rendered manifest by these experiments. It is not the strings of a harp, or a lute, or a piano, or a violin, that throw the air into sonorous vibrations. It is the large surfaces with which the strings are associated, and the air enclosed by these surfaces. The goodness of such instruments depends almost

wholly upon the quality and disposition of their sound boards.[1]

Take the violin as an example. It is, or ought to be, formed of wood of the most perfect elasticity. Imperfectly elastic wood expends the motion imparted to it in the friction of its own molecules; the motion is converted into heat, instead of sound. The strings of the violin pass from the 'tail-piece' of the instrument over the 'bridge,' being thence carried to the 'pegs,' the turning of which regulates the tension of the strings. The bow is drawn across at a point about one-tenth of the length of the string from the bridge. The two

Fig. 33.

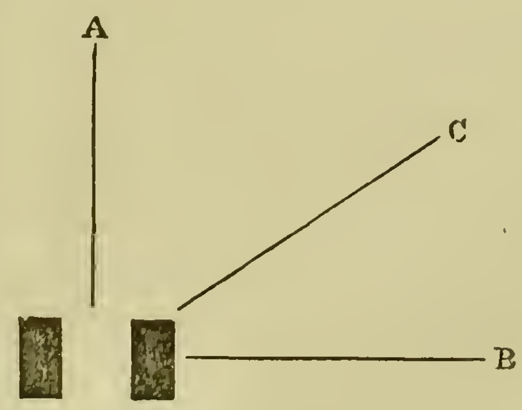


[1] To show the influence of a large vibrating surface in communicating sonorous motion to the air, Mr. Kilburn encloses a musical box within cases of thick felt. Through the cases a wooden rod, which rests upon the box, issues. When the box plays a tune, it is unheard as long as the rod only emerges; but when a thin disc of wood is fixed on the rod, the music becomes immediately audible.

'feet' of the bridge rest upon the most yielding portion of the 'belly' of the violin—that is, the portion that lies between the two *f* shaped orifices. One foot is fixed over a short rod, the 'sound post,' which runs from belly to back through the interior of the violin. The foot of the bridge is thereby rendered rigid, and it is mainly through the other foot, which is not thus supported, that the vibrations are conveyed to the wood of the instrument, and thence to the air within and without. The sonorous quality of the wood of a violin is mellowed by age. The very act of playing also has a beneficial influence, apparently constraining the molecules of the wood, which in the first instance might be refractory, to conform at last to the requirements of the vibrating strings.

Professor Stokes has applied to sound-boards the conception which enabled him to explain the observation of Sir John Leslie regarding the action of hydrogen on sound (page 8). When a string vibrates alone the air slips readily to and fro round so small a body, abolishing the condensations and rarefactions. But this is not accomplished so easily when the vibrating body is of considerable area. The air cannot then move away in front nor slip in behind before it is sensibly condensed or rarefied. Hence with such vibrating bodies sound-waves may be generated and loud tones produced, while the thin strings that set them in vibration, acting alone, are quite inaudible.

Fig. 34.



The increase of sound produced by the stoppage of lateral motion has experimentally illustrated by Professor Stokes. Let the two black rectangles in fig. 34 represent the section of the

prongs of a tuning-fork. After it has been made to vibrate, place a sheet of paper or the blade of a broad knife, with its edge parallel to the axis of the fork, and as near to the fork as may be without touching. If the obstacle be so placed that the section of it is A or B, no effect is produced; but if it be placed at C, so as to prevent the reciprocating to and fro movement of the air, which tends to abolish the condensations and rarefactions, the sound becomes much stronger.

§ 2. *Laws of Vibrating Strings.*

Having thus learned how the vibrations of strings are rendered available in music, we have next to investigate the laws of such vibrations. I pluck at its middle point the string B B′, fig. 31. The sound heard is the fundamental or lowest note of the string, to produce which it swings, as a whole, to and fro. By placing a moveable bridge under the middle of the string, and pressing the string against the bridge, it is divided into two equal parts. Plucking either of those at its centre, a musical note is obtained, which many of you recognise as the octave of the fundamental note. In all cases, and with all instruments, the octave of a note is produced by doubling the number of its vibrations. It can, moreover, be proved, both by theory and by the syren, that this half string vibrates with exactly twice the rapidity of the whole. In the same way it can be proved that one-third of the string vibrates with three times the rapidity, producing a note a fifth above the octave, while one-fourth of the string vibrates with four times the rapidity, producing the double octave of the whole string. In general terms, *the number of vibrations is inversely proportional to the length of the string.* This is our first law.

Again, the more tightly a string is stretched the more rapid is its vibration. When this comparatively slack string is caused to vibrate, you hear its low fundamental

note. By turning a peg, round which one end of it is coiled, the string is tightened, and the pitch rendered higher. Taking hold with my left hand of the weight W, attached to the wire B B′ of our sonometer, and plucking the wire with the fingers of my right, I alternately press upon the weight and lift it. The quick variations of tension are expressed by a varying wailing tone. Now, the number of vibrations executed in the unit of time bears a definite relation to the stretching force. Applying different weights to the end of the wire B B′, and determining in each case the number of vibrations executed in a second, we find the numbers thus obtained to be *proportional to the square roots of the stretching weights*. A string, for example, stretched by a weight of 1 lb., executes a certain number of vibrations per second; if we wish to double this number, we must stretch it by a weight of 4 lbs.; if we wish to treble the number, we must apply a weight of 9 lbs., and so on. This is our second law.

The vibrations of a string also depend upon its thickness. Preserving the stretching weight, the length, and the material of the string constant, *the number of vibrations varies inversely as the thickness of the string*. If, therefore, of two strings of the same material, equally long and equally stretched, the one has twice the diameter of the other, the thinner string will execute double the number of vibrations of its fellow in the same time. If one string be three times as thick as another, the latter will execute three times the number of vibrations, and so on. This is our third law.

Finally, the vibrations of a string depend upon the density of the matter of which it is composed. A platinum wire and an iron wire, for example, of the same length and thickness, stretched by the same weight, will not vibrate with the same rapidity. For while the specific gravity of iron, or in other words its density, is 7·8, that of platinum

is 21·5. All other conditions remaining the same, *the number of vibrations is inversely proportional to the square root of the density of the string.* If the density of one string, therefore, be one-fourth that of another of the same length, thickness, and tension, it will execute its vibrations twice as rapidly; if its density be one-ninth that of the other, it will vibrate with three times the rapidity, and so on. This is our fourth law. The third and fourth laws, taken together, may be expressed thus:—*The number of vibrations is inversely proportional to the square root of the weight of the string.*

In the violin and other stringed instruments we avail ourselves of thickness instead of length to obtain the deeper tones. In the piano we not only augment the thickness of the wires intended to produce the bass notes, but we load them by coiling round them an extraneous substance. They resemble horses heavily jockeyed, and move more slowly on account of the greater weight imposed upon the force of tension.

§ 3. *Mechanical Illustrations of Vibrations. Progressive and Stationary Waves. Ventral Segments and Nodes.*

These, then, are the four laws which regulate the *transverse* vibrations of strings. We now turn to certain allied phenomena, which, though they involve mechanical considerations of a rather complicated kind, may be completely mastered by an average amount of attention. And they *must* be mastered if we would thoroughly comprehend the philosophy of stringed instruments.

From the ceiling *c*, fig. 35, of this room hangs an india-rubber tube 28 feet long. The tube is filled with sand to render its motions slow and more easily followed by the eye. I take hold of its free end *a*, stretch the tube a little, and by properly timing my impulses cause it to

swing to and fro as a whole, as shown in the figure. It has its definite period of vibration dependent on its length, weight, thickness, and tension, and my impulses must synchronise with that period.

I now stop the motion, and by a sudden jerk raise a hump upon the tube, which runs along it as a pulse towards its fixed end; here the hump reverses itself, and runs back to my hand. At the fixed end of the tube, in obedience to the law of reflection, the pulse reversed both its position and the direction of its motion. Supposing *c*, fig. 36, to be the fixed end of the tube, and *a* the end held in the hand; if the pulse on reaching *c* have the position shown in (1), after reflection it will have the position shown in (2). The arrows mark the direction of progression. The time required for the pulse to pass from the hand to the fixed end and back is exactly that required to accomplish one complete vibration of the tube as a whole. It is indeed the addition of such impulses which causes the tube to continue to vibrate as a whole.

FIG. 35.

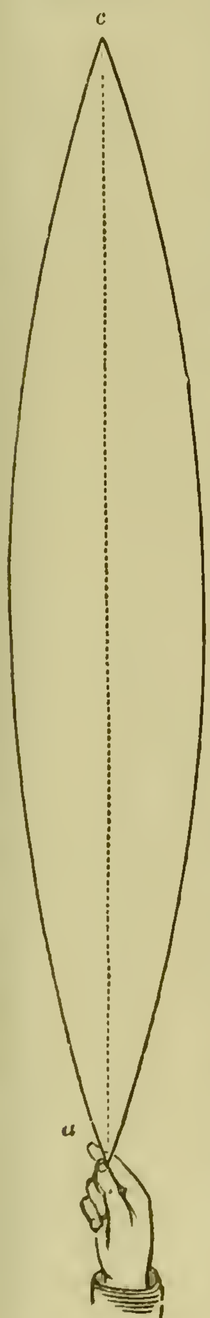


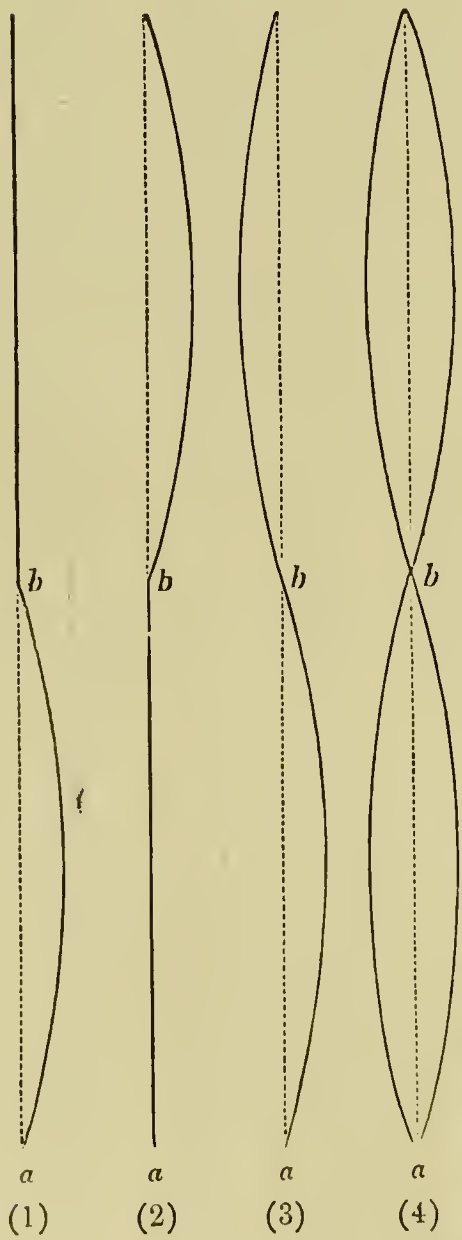
FIG. 36.

If, instead of a single jerk, a succession of jerks be imparted, thereby sending a series of pulses along the tube, every one of them will be reflected above, and we

have now to inquire how the direct and reflected pulses behave towards each other.

Let the time required by the pulse to pass from the hand to the fixed end be one second; at the end of half a second it occupies the position *a b* (1), fig. 37, its foremost point having reached the middle of the tube. At the end of a whole second it would have the position *b c* (2), its foremost point having reached the fixed end *c* of the tube. At the moment when reflection begins at *c*, let another jerk be imparted at *a*. The reflected pulse from *c* moving with the same velocity as this direct one from *a*, the foremost points of both will arrive at the centre *b* (3) at the same moment. What must occur? The hump *a b* wishes to move on to *c*, and to do so must move the point *b* to the right. The hump *c b* wishes to move towards *a*, and to do so must move the point *b* to the left. The point *b*, urged by equal forces in two opposite directions at the same time, will not move in either direction. Under these circumstances, the two halves *a b*, *b c* of the tube will oscillate as if they were independent of each other (4). Thus by the combination of two *progressive pulses*, the one direct and the other reflected, we produce two *stationary pulses* on the tube *a c*.

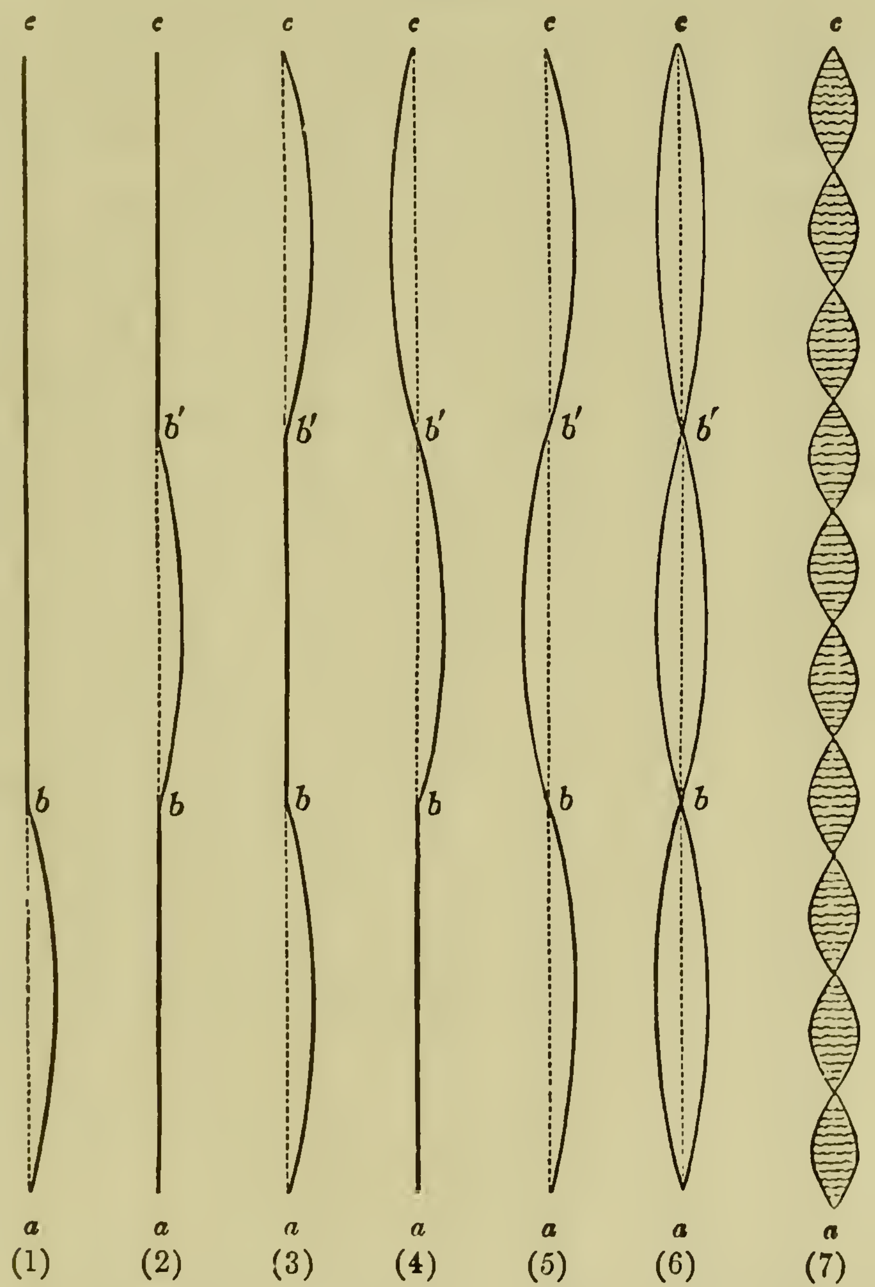
Fig. 37.



The vibrating parts *a b* and *o c* are called *ventral segments*; the point of no vibration *b* is called a *node.*

The term 'pulse' is here used advisedly, instead of the more usual term *wave.* For a wave embraces two of these pulses. It embraces both the hump and the depres-

Fig. 38.



sion which follows the hump. The length of a wave, therefore, is twice that of a ventral segment.

Suppose the jerks to be so timed as to cause each hump to be one third of the tube's length. At the end of one-third of a second from starting the pulse will be in

the position $a\ b$ (1), fig. 38 (previous page). In two-thirds of a second it will have reached the position $b\ b'$ (2), fig. 38. At this moment let a new pulse be started at a; after the lapse of an entire second from the commencement we shall have two humps upon the tube, one occupying the position $a\ b$ (3), the other the position $b'\ c$ (3). It is here manifest that the end of the reflected pulse from c, and the end of the direct one from a, will reach the point b' at the same moment. We shall therefore have the state of things represented in (4), where $b\ b'$ wishes to move upwards, and $c\ b'$ to move downwards. The action of both upon the point b' being in opposite directions, that point will remain fixed. *And from it, as if it were a fixed point, the pulse $b\ b'$ will be reflected, while the segment $b'\ c$ will oscillate as an independent string.* Supposing that at the moment $b\ b'$ (4) begins to be reflected at b', we start another pulse from a, it will reach b (5) at the same moment the pulse reflected from b' reaches it. The pulses will neutralise each other at b, and we shall have there a second node. Thus, by properly timing our jerks, we divide the rope into three ventral segments, separated from each other by two nodal points. As long as the agitation continues the tube will vibrate as in (6).

There is no theoretic limit to the number of nodes and ventral segments that may be thus produced. By the quickening of the impulses, the tube is divided into four ventral segments separated by three nodes; quickening still more we have five ventral segments and four nodes. With this particular tube the hand may be caused to vibrate sufficiently quick to produce ten ventral segments, as shown in fig. 38 (7). When the stretching force is constant, the number of ventral segments is proportional to the rapidity of the hand's vibration. To produce 2, 3, 4, 10 ventral segments requires twice, three times, four times, ten times the rapidity of vibration necessary

to make the tube swing as a whole. When the vibration is very rapid the ventral segments appear like a series of shadowy spindles, separated from each other by dark motionless nodes. The experiment is a beautiful one, and it is easily performed.

If, instead of moving the hand to and fro, it be caused to describe a small circle, the ventral segments become ‘surfaces of revolution.’ Instead of the hand, moreover, we may employ a hook turned by a whirling-table. Before you is a cord more rigid than the india-rubber tube, 25 feet long, with one of its ends attached to a freely-moving swivel fixed in the ceiling of the room. By turning the whirling-table to which the other end is attached, this cord may be divided into as many as 20 ventral segments, separated from each other by their appropriate nodes. In another arrangement a string of catgut 12 feet long, with silvered beads strung along it, is stretched horizontally between a vertical wheel and a free swivel fixed in a rigid stand. On turning the wheel, and properly regulating both the tension and the rapidity of rotation, the beaded cord may be caused to rotate as a whole, and to divide itself successively into 2, 3, 4, or 5 ventral segments. When we envelop the cord in a luminous beam, every spot of light on every bead describes a brilliant circle, and a very beautiful experiment is the result.

§ 4. *Mechanical Illustrations of damping various points of Vibrating Cord.*

The subject of *stationary waves* was first experimentally treated by the Messrs. Weber, in their excellent researches on Wave-motion, published in the ‘Wellenlehre.’ It is a subject which will well repay your attention by rendering many of the most difficult phenomena of musical strings perfectly intelligible. The connection of both

classes of vibrations will be more obvious if we vary our last experiments. Before you is a piece of india-rubber tubing, 10 or 12 feet long, stretched from *c* to *a*, fig. 39, and made fast to two pins at *c* and *a*. The tube is blackened, and behind it is placed a surface of white paper, to

Fig. 39.

render its motions more visible. Encircling the tube at its centre *b* (1) by the thumb and fore-finger of my left hand, and taking the middle of the lower half *b a* of the tube in my right, I pluck it aside. Not only does the lower half swing, but the upper half also is thrown into

vibration. Withdrawing the hands wholly from the tube, its two halves *a b* and *b c* continue to vibrate, being separated from each other by a node *b* at the centre (2).

I now encircle the tube at a point *b* (3) one-third of its length from its lower end *a*, and taking hold of *a b* at its centre, pluck it aside; the length *b c* above my hand instantly divides into two vibrating segments. Withdrawing the hands wholly, you see the entire tube divided into three ventral segments, separated from each other by two motionless nodes *b* and *b′* (4). I pass on to the point *b* (5), which marks off one-fourth of the length of the tube, encircle it, and pluck the shorter segment aside. The longer segment above my hand divides itself immediately into three vibrating parts. So that, on withdrawing the hand, the whole tube appears before you divided into four ventral segments, separated from each other by three nodes *b b′ b″* (6). In precisely the same way the tube may be divided into five vibrating segments with four nodes.

This sudden division of the long upper segment of the tube, without any apparent cause, is very surprising; but if you grant me your attention for a moment, you will find that these experiments are essentially similar to those which illustrated the coalescence of direct and reflected undulations. Reverting for a moment to the latter (p. 92), you observed that the to-and-fro motion of the hand through the space of a single inch was sufficient to make the middle points of the ventral segments vibrate through a foot or eighteen inches. By being properly timed the impulses accumulated, until the amplitude of the vibrating segments exceeded immensely that of the hand which produced them. The hand, in fact, constituted a nodal point, so small was its comparative motion. Indeed, it is usual, and correct, to regard the ends of the tube also as nodal points.

Consider now the case represented in (1) fig. 39, where the tube was encircled at its middle, the lower segment $a\ b$ being thrown into the vibration corresponding to its length and tension. The circle formed by the finger and thumb permitted the tube to oscillate at the point b through the space of an inch; and the vibrations at that point acted upon the upper half $b\ c$ exactly as my hand acted when it caused the tube suspended from the ceiling to swing as a whole, as in fig. 35. Instead of the timed vibrations of the hand, we have now the timed vibrations of the lower half of the tube; and these, though narrowed to an inch at the place clasped by the finger and thumb, soon accumulate, and finally produce an amplitude, in the upper half, far exceeding their own. The same reasoning applies to all the other cases of subdivision. If, instead of encircling a point by the finger and thumb and plucking the portion of the tube below it, that same point were taken hold of by the hand and agitated in the period proper to the lower segment of the tube, precisely the same effect would be produced. We thus reduce both effects to one and the same cause; namely, the combination of direct and reflected undulations.

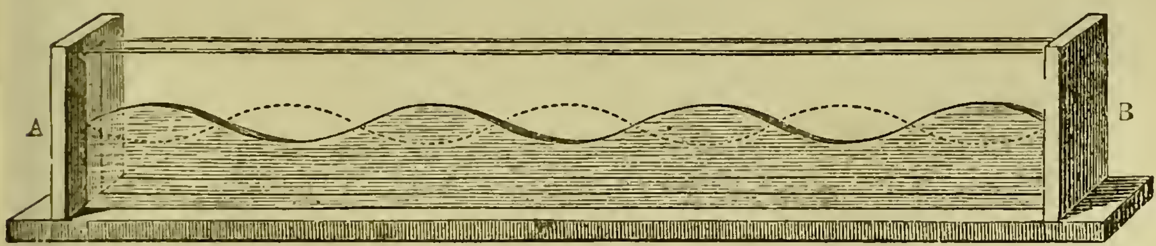
And here, let me add, that when the tube was divided by the timed impulses of the hand, not one of its nodes was, strictly speaking, a point of no motion; for were the nodes not capable of vibrating through a very small amplitude, the motion of the various segments of the tube could not be maintained.

§ 5. *Stationary Water-waves.*

What is true of the undulations of an india-rubber tube applies to all undulations whatsoever. Water-waves, for example, obey the same laws, and the coalescence of direct and reflected waves exhibit similar phenomena. This long and narrow vessel with glass sides, fig. 40, is a

copy of the wave canal of the brothers Weber. It is filled to the level A B with coloured water. By tilting the end A suddenly, a wave is generated, which moves on to B, and is there reflected. By sending forth fresh waves at the proper intervals, the surface is divided into two stationary undulations. Making the succession of impulses more rapid we can subdivide the surface into three, four (shown in the figure), or more stationary undulations, separated from each other by nodes. The step of a water-carrier is sometimes so timed as to throw the surface of the water in his vessel into stationary waves, which may augment in height until the water splashes over the brim. Practice

FIG. 40.



has taught the water-carrier what to do; he changes his step, alters the period of his impulses, and thus stops the accumulation of the motion

While travelling, some years ago,[1] in the coupé of a French railway carriage I had occasion to place a bottle half filled with water on one of the little coupé tables. My companion observed it with thoughtful interest. At times it would be quite still; at times it would oscillate violently. To the passenger within the carriage there was no sensible change in the motion of the train to which the difference could be ascribed. But in the one case the tremor of the carriage contained no vibrations synchronous with the oscillating period of the water, while in the other case such vibrations were present. Out of the confused assemblage of tremors the water selected the

[1] In company with Mr. Thomas Carlyle.

particular constituent which belonged to itself, and declared its presence when the traveller was utterly unconscious of its introduction.

§ 6. *Application of Mechanical Illustrations to Musical Strings.*

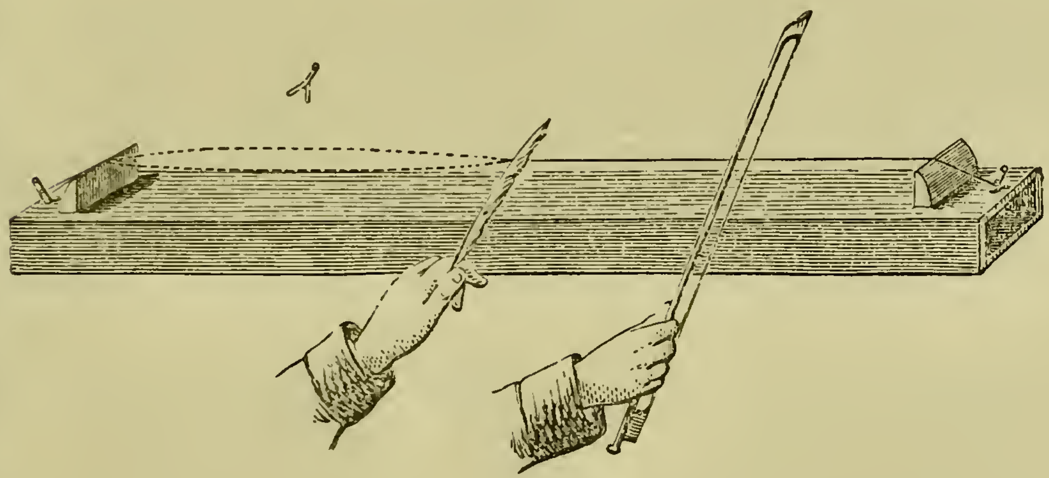
From these comparatively gross, but by no means unbeautiful, mechanical vibrations, we pass to those of a sounding string. In the experiments with our monochord, when the wire was to be shortened, a movable bridge was employed, against which the wire was pressed so as to deprive the point resting on the bridge of all possibility of motion. This strong pressure, however, is not necessary. Placing the feather end of a goose-quill lightly against the middle of the string, and drawing a violin bow over one of its halves, the string yields the octave of the note yielded by the whole string. The mere *damping* of the string at the centre, by the light touch of the feather, is sufficient to cause the string to divide into two vibrating segments. Nor is it necessary to hold the feather there throughout the experiment: after having drawn the bow, the feather may be removed; the string will continue to vibrate, emitting the same note as before. We have here a case exactly analogous to that in which the central point of our stretched india-rubber tube was damped, by encircling it with the finger and thumb as in fig. 39 (1). Not only did the half plucked aside vibrate, but the other half vibrated also. We can, in fact, reproduce, with the vibrating string, every effect obtained with the tube. This, however, is a point of such importance as to demand full experimental illustration.

To prove that when the centre is damped, and the bow drawn across one of the halves of the string, the other half vibrates, I place across the middle of the untouched

half a little rider of red paper. Damping the centre and drawing the bow, the string shivers, and the rider is overthrown, fig. 41.

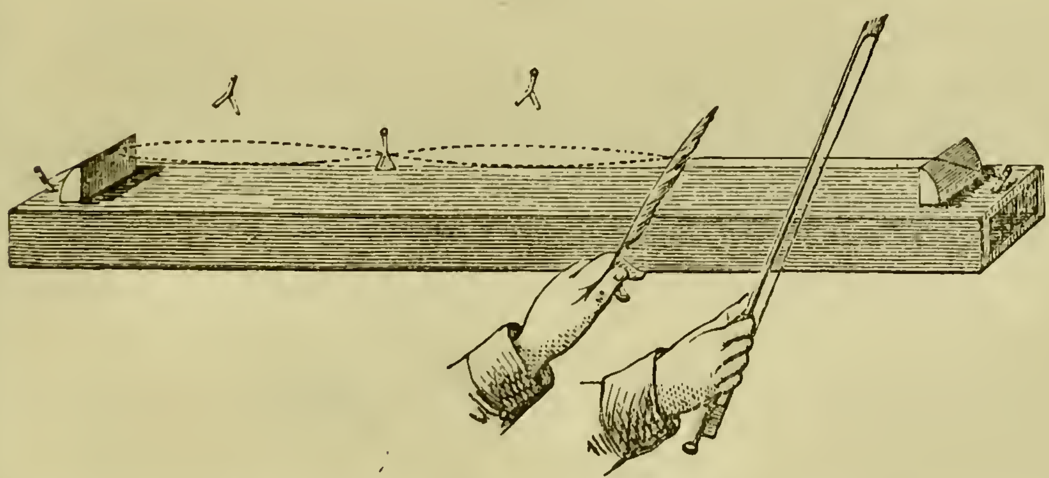
When the string is damped at a point which cuts off one-third of its length, and the bow drawn across the

FIG. 41.



shorter section, not only is this section thereby thrown into vibration, but the longer section divides itself into two ventral segments with a node between them. This is proved by placing small riders of red paper on the ventral

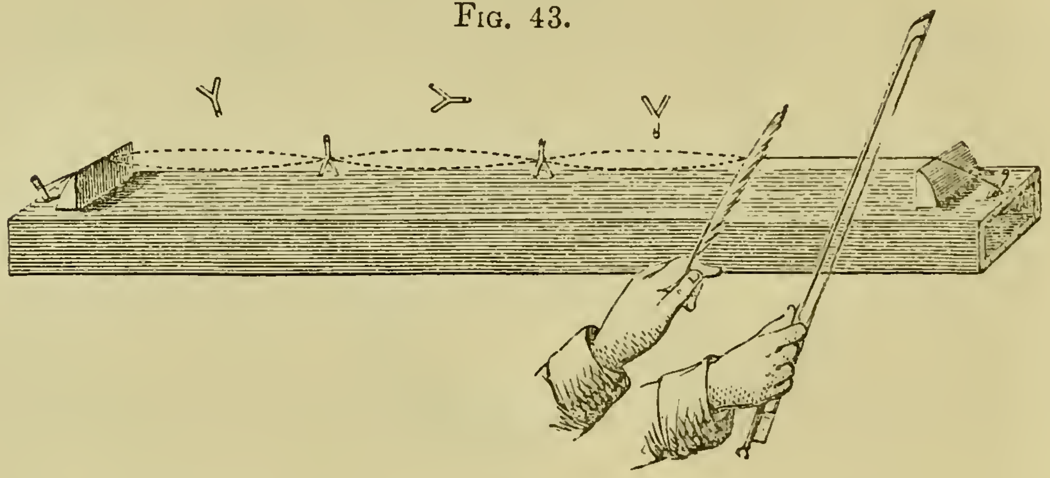
FIG. 42.



segments, and a rider of blue paper at the node. Passing the bow across the short segment you observe a fluttering of the red riders, and now they are completely tossed off, while the blue rider which crosses the node is undisturbed, fig. 42.

Damping the string at the end of one-fourth of its length, the bow is drawn across the shorter section; the remaining three-fourths divide themselves into three ventral segments, with two nodes between them. This is proved by the unhorsing of the three riders placed astride

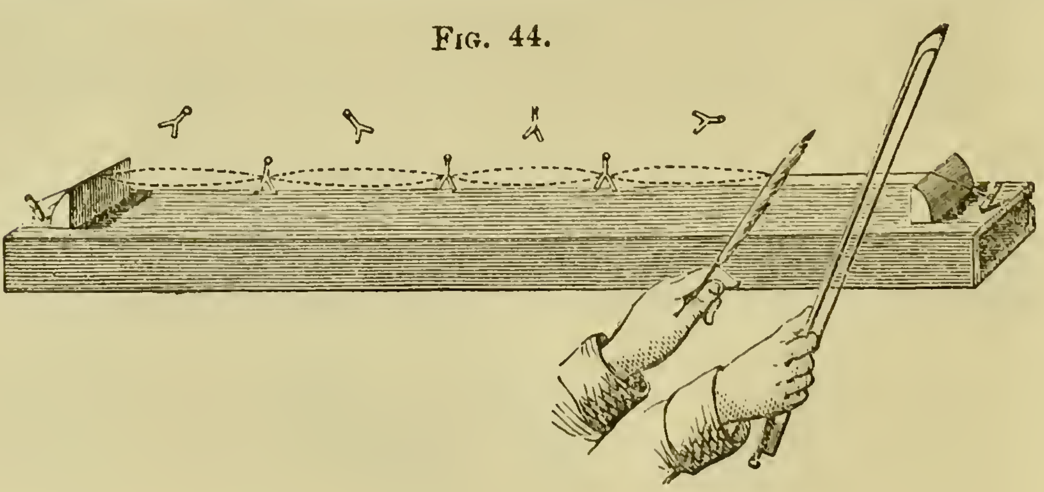
Fig. 43.



the ventral segments, the two at the nodes keeping their places undisturbed, fig. 43.

Finally, damping the string at the end of one-fifth of its length, and arranging, as before, the red riders on the

Fig. 44.



ventral segments and the blue ones on the nodes; by a single sweep of the bow the four red riders are unhorsed, and the three blue ones left undisturbed, fig. 44. In this way we perform with a sounding string the same series of experiments that were formerly executed with a stretched

india-rubber tube, the results in both cases being identical.[1]

To make, if possible, this identity still more evident to you, a stout steel wire 28 feet in length is stretched behind the table from side to side of the room. I take the central point of this wire between my finger and thumb, and allow my assistant to pluck one-half of it aside. It vibrates, and the vibrations transmitted to the other half are sufficiently powerful to toss into the air a large sheet of paper placed astride the wire. With this long wire, and with riders not of one-eighth of a square inch, but of 30, 40, or 50 square inches in area, we may repeat all the experiments which you have witnessed with the musical string. The sheets of paper placed across the nodes remain always in their places, whilst those placed astride the ventral segments are tossed simultaneously into the air when the shorter segment of the wire is set in vibration. In this case, when close to it, you can actually see the division of the wire.

§ 7. *Melde's Experiments.*

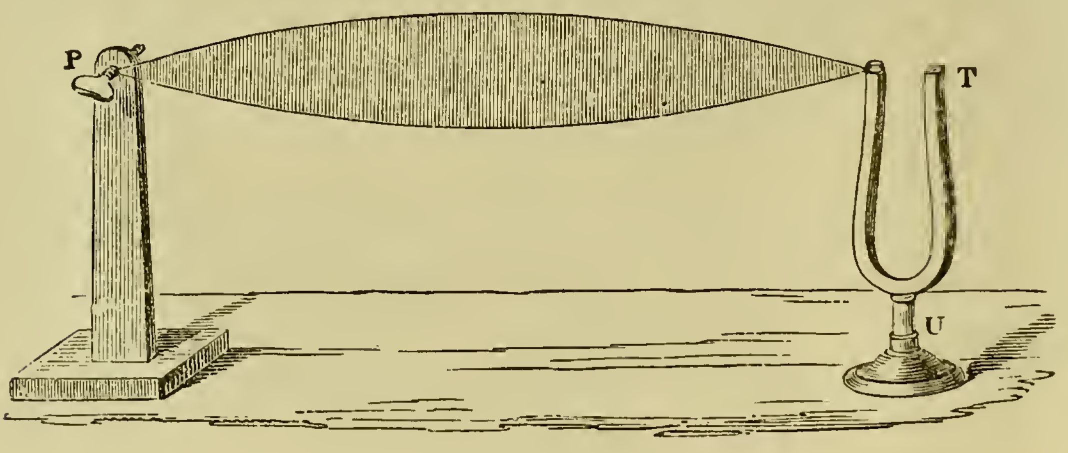
It is now time to introduce to your notice some recent experiments on vibrating strings, which appeal to the eye with a beauty and a delicacy far surpassing anything attainable with our monochord. To M. Melde, of Marburg, we are indebted for this new method of exhibiting the vibrations of strings. The scale of the experiments will be here modified so as to suit our circumstances.

First, then, you observe here a large tuning-fork T,

[1] Chladni remarks (*Akustik*, p. 55) that it is usual to ascribe to Sauveur the discovery, in 1701, of the modes of vibration corresponding to the higher tones of strings; but that Noble and Pigott had made the discovery in Oxford in 1676, and that Sauveur declined the honour of the discovery when he found that others had made the observation before him.

fig. 45, with a small screw fixed into the top of one of its prongs, by which a silk string can be firmly attached to the prong. From the fork the string passes round a distant peg P, by turning which it may be stretched to any required extent. When the bow is drawn across the fork, an irregular flutter of the string is the only result. On tightening it, however, when at the proper tension it expands into a beautiful gauzy spindle six feet long, more than six inches across at its widest part, and shining with a kind of pearly lustre. The stretching force at the pre

Fig. 45.



sent moment is such that the string swings to and fro as a whole, its vibrations being executed in a vertical plane.

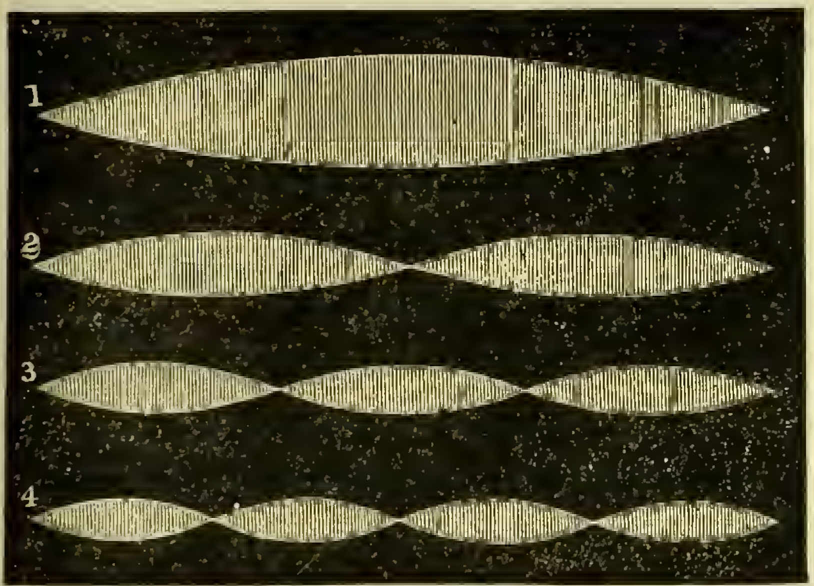
Relaxing the string gradually, when the proper tension has been reached, it suddenly divides into two ventral segments, separated from each other by a sharply-defined and apparently motionless node.

While the fork continues vibrating, if the string be relaxed still further, it divides into three vibrating parts. Slackening it still more, it divides into four vibrating parts. And thus we might continue to subdivide the string into ten, or even twenty ventral segments, separated from each other by the appropriate number of nodes.

When white silk strings vibrate thus, the nodes appear

perfectly fixed, while the ventral segments form spindles of the most delicate beauty. Every protuberance of the twisted string, moreover, writes its motion in a more or

FIG. 46.



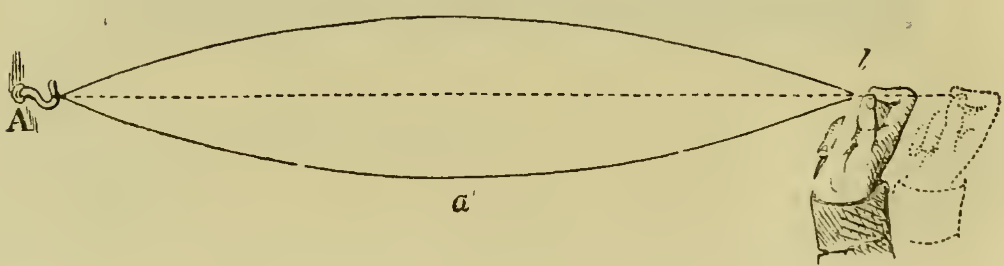
less luminous line on the surface of the aerial gauze. The four modes of vibration just illustrated are represented in fig. 46, 1, 2, 3, 4.[1]

[1] The first experiment really made in the lecture was with a bar of steel 62 inches long, 1½ inch wide, and half an inch thick, bent into the shape of a tuning-fork, with its prongs 2 inches apart, and supported on a heavy stand. The cord attached to it was 9 feet long and a quarter of an inch thick. The prongs were thrown into vibration by striking them briskly with two pieces of lead covered with pads and held one in each hand. The prongs vibrated transversely to the cord. The vibrations produced by a single stroke were sufficient to carry the cord through several of its subdivisions and back to a single ventral segment. That is to say, by striking the prongs and causing the cord to vibrate as a whole, it could, by relaxing the tension, be caused to divide into two, three, or four vibrating segments; and then, by increasing the tension, to pass back through four, three, and two divisions, to one, *without renewing the agitation of the prongs.* The cord was of such a character that, instead of oscillating to and fro in the same plane, each of its points described a circle. The ventral segments, therefore, instead of being flat surfaces, were surfaces of revolution, and were equally well seen from all parts of the room. The tuning-forks employed in the subsequent illustrations were prepared for me by that excellent acoustic mechanician, König, of Paris, being such as are usually employed in the projection of Lissajous' experiments.

When the synchronism between fork and string is perfect, the vibrations of the string are steady and long-continued. A slight departure from synchronism, however, introduces unsteadiness, and the ventral segments, though they may show themselves for a time, quickly disappear.

In the experiments just executed, the fork vibrated in the direction of the length of the string. Every forward stroke of the fork raised a protuberance, which ran to the fixed end of the string, and was there reflected; so that when the *longitudinal* impulses were properly timed they produced a *transverse* vibration. Let us consider this further. One end of this heavy cord is attached to a

FIG. 47.

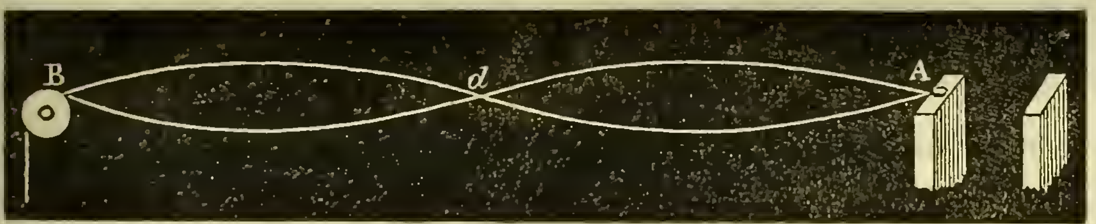


hook A, fig. 47, fixed in the wall. Laying hold of the other end I stretch the cord horizontally, and then move my hand to and fro in the direction of the cord. It swings as a whole, and you may notice that always, when the cord is at the limit of its swing, the hand is in its most forward position. If it vibrate in a vertical plane, the hand, in order to time the impulses properly, must be at its forward limit l at the moment the cord reaches the upper boundary a, and also at the moment it reaches the lower boundary a' of its excursion. A little reflection will make it plain that, in order to accomplish this, the hand must execute a complete vibration while the cord executes a semi-vibration; in other words, the vibrations of the hand must be twice as rapid as those of the cord.

Precisely the same is true of our tuning-fork. When the fork vibrates in the direction of the string, the number of vibrations which it executes in a certain time is twice the number executed by the string itself. And if, while arranged thus, a fork and string vibrate with sufficient rapidity to produce musical notes, the note of the fork will be an octave above that of the string.

But if, instead of the hand being moved to and fro in the direction of this heavy cord, it is moved at right angles to that direction, then every upward movement of the hand coincides with an upward movement of the cord; every downward movement of the hand with a downward movement of the cord. In fact, the vibrations of hand

FIG. 48.



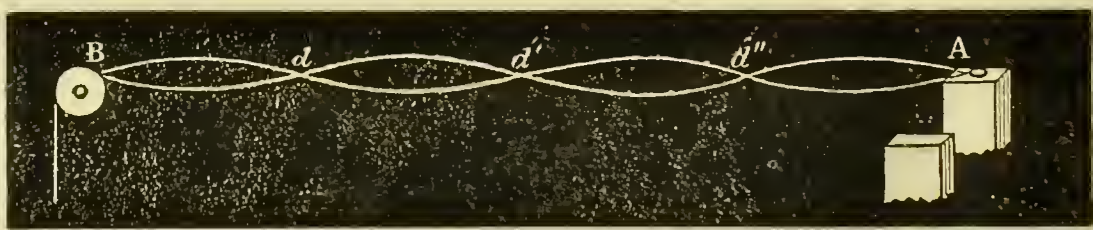
and string, in this case, synchronise perfectly; and if the hand could emit a musical note, the cord would emit a note of the same pitch. The same holds good when a vibrating fork is substituted for the vibrating hand.

Hence, if the string vibrate as a whole when the vibrations of the fork are *along* it, it will divide into two ventral segments when the vibrations are *across* it; or, more generally expressed, preserving the tension constant, whatever be the number of ventral segments produced by the fork when its vibrations are in the direction of the string, twice that number will be produced when the vibrations are transverse to the string. The string A B, for example, figs. 48 and 49, passing over a pulley B, is stretched by a definite weight (not shown in the figure). When the tuning-fork vibrates *along* it, as in fig. 48, the string

divides into two equal ventral segments. When the fork is turned so that it shall vibrate at right angles to the string, fig. 49, the number of ventral segments is four, or double the former number. Attaching two strings of the same length to the same fork, the one parallel and the other perpendicular to the direction of vibration, and stretching both with equal weights; when the fork is caused to vibrate, one of them divides itself into twice the number of ventral segments exhibited by the other.

A number of exquisite effects may be obtained with these vibrating cords. The path described by any point of any one of them may be studied, after the manner of

Fig. 49.



Dr. Young, by illuminating that point, and watching the line of light which it describes. This is well illustrated by a flat burnished silver wire, twisted so as to form a spiral surface, from which, at regular intervals, the light flashes when the wire is illuminated. When the vibration is steady the luminous spots describe straight lines of sunlike brilliancy. On slackening the wire, but not so much as to produce its next higher subdivision, upon the larger motion of the wire are superposed a host of minor motions, the combination of all producing scrolls of marvellous complication and of indescribable beauty.

In reflecting on the best means of rendering these effects visible, the thought occurred to me of employing a fine platinum wire heated to redness by an electric current. Such a wire now stretches from a tuning-fork over a bridge of copper, and then passes round a peg. The copper

bridge on the one hand and the tuning-fork on the other are the poles of a voltaic battery, from which a current passes through the wire and causes it to glow. On drawing the bow across the fork, the wire vibrates as a whole; its two ends are brilliant, while its middle is dark, being chilled by its rapid passage through the air. Thus you have a shading-off of incandescence from the ends to the centre of the wire. On relaxing the tension, the wire divides itself into two ventral segments: on relaxing still further, we obtain three; still further, and the wire divides into four ventral segments, separated from each other by three incandescent nodes. Right and left from every node the incandescence shades away until it disappears. You notice also, when the wire settles into steady vibration, that the nodes shine out with greater brilliancy than that possessed by the wire before the vibration commenced. The reason is this. Electricity passes more freely along a cold wire than along a hot one. When, therefore, the vibrating segments are chilled by their swift passage through the air, their conductivity is improved, more electricity passes through the vibrating than through the motionless wire, and hence the augmented glow of the nodes. If, previous to the agitation of the fork, the wire be at a bright red heat, when it vibrates its nodes may be raised to the temperature of fusion.

§ 8. *New Mode of determining the Laws of Vibration.*

We may extend the experiments of M. Melde to the establishment of all the laws of vibrating strings. Here are four tuning-forks, which we may call *a*, *b*, *c*, *d*, whose rates of vibration are to each other as the numbers 1, 2, 4, 8. To the largest fork is attached a string, *a*, stretched by a weight, which causes it to vibrate as a whole. Keeping the stretching weight the same, I determine the lengths

of the same string, which, when attached to the other three forks, *b*, *c*, *d*, swing as a whole. The lengths in the four respective cases are as the numbers 8, 4, 2, 1.

From this follows the first law of vibration, already established (p. 88) by another method, viz:—*the length of the string is inversely proportional to the rapidity of vibration.*[1]

In this case the longest string vibrates as a whole when attached to the fork *a*. I now transfer the string to *b*, still keeping it stretched by the same weight. It vibrates when *b* vibrates; but how? By dividing into two equal ventral segments. In this way alone can it accommodate itself to the swifter vibrating period of *b*. Attached to *c*, the same string separates into four, while when attached to *d* it divides into eight ventral segments. The number of the ventral segments is proportional to the rapidity of vibration. It is evident that we have here, in a more delicate form, a result which we have already established in the case of our india-rubber tube set in motion by the hand. It is also plain that this result might be deduced theoretically from our first law.

We may extend the experiment. Here are two tuning-forks separated from each other by the musical interval called a fifth. Attaching a string to one of the forks, I stretch the string until it divides into two ventral segments: attached to the other fork, and stretched by the same weight, it divides instantly into three segments when the fork is set in vibration. Now, to form the interval of a fifth, the vibrations of the one fork must be to those of the other in the ratio of 2 : 3. The division of the string, therefore, declares the interval. In the same way the

[1] A string steeped in a solution of the sulphate of quinine, and illuminated by the violet rays of the electric lamp, exhibits brilliant fluorescence. When the fork to which it is attached vibrates, the string divides itself into a series of spindles, and separated from each other by more intensely luminous nodes, emitting a light of the most delicate greenish-blue.

division of the string in relation to all other musical intervals may be illustrated.[1]

Again. Here are two tuning-forks, a and b, one of which (a) vibrates twice as rapidly as the other. A string of silk is attached to a, and stretched until it synchronises with the fork, and vibrates as a whole. Here is a second string of the same length, formed by laying four strands of the first one side by side. I attach this compound thread to b, and keeping the tension the same as in the last experiment, set b in vibration. The compound thread synchronises with b, and swings as a whole. Hence, as the fork b vibrates with half the rapidity of a, by quadrupling the weight of the string we halve its rapidity of vibration. In the same simple way it might be proved that by augmenting the weight of the string nine times we reduce the number of its vibrations to one-third. We thus demonstrate the law :—

The rapidity of vibration is inversely proportional to the square root of the weight of the string.

An instructive confirmation of this result is thus obtained :—Attached to this tuning-fork is a silk string six feet long. Two feet of the string are composed of four strands of the single thread, placed side by side, the remaining four feet are a single thread. A stretching force is applied, which causes the string to divide into two ventral segments. But how does it divide? Not at its centre, as is the case when the string is of uniform thickness throughout, but at the precise point where the thick string terminates. This thick segment, two feet long, is now vibrating at the same rate as the thin segment four feet long, a result which follows by direct deduction from the two laws already established.

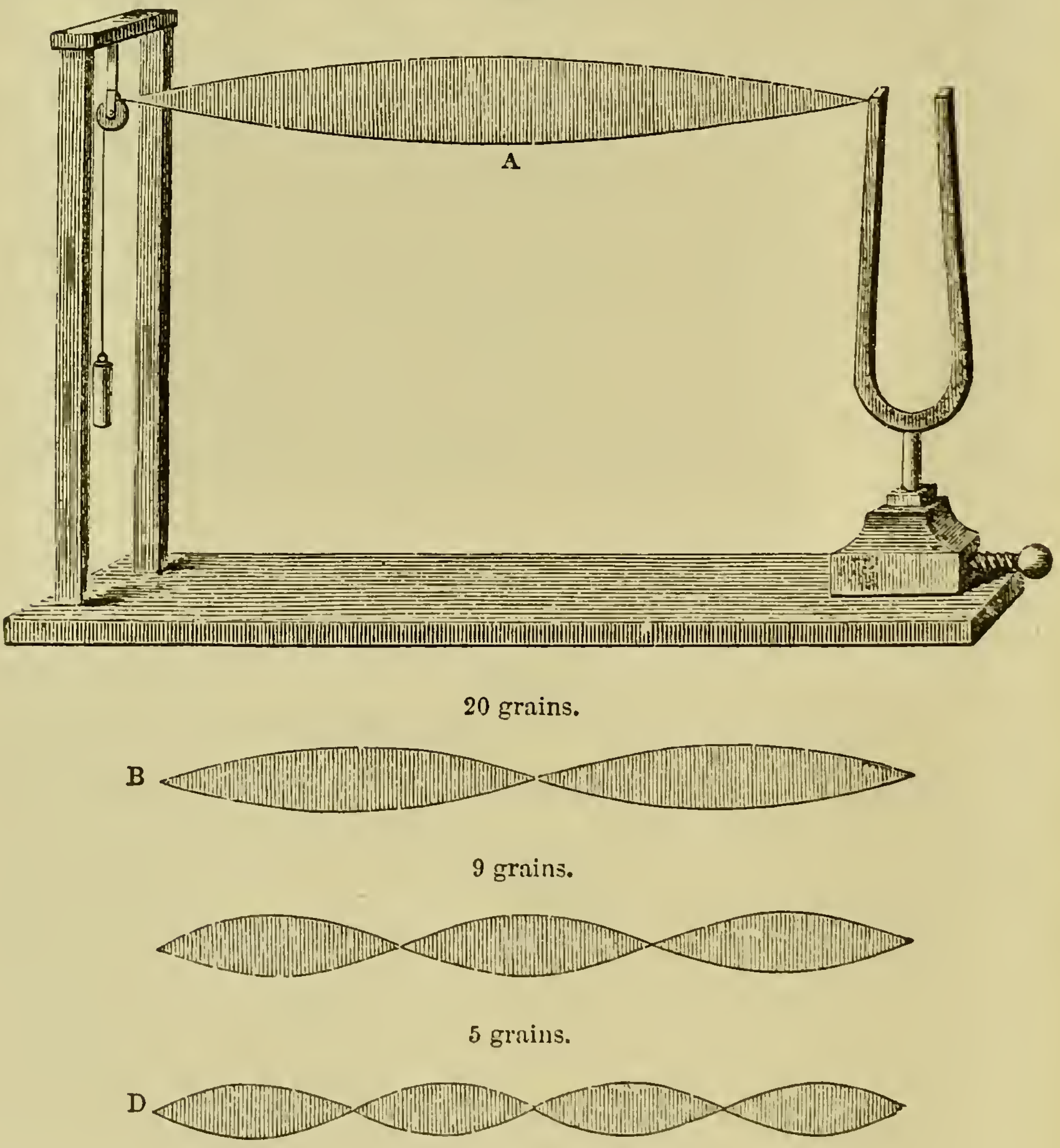
Here again are two strings of the same length and

[1] The subject of musical intervals will be treated in a subsequent lecture.

thickness. One of them is attached to the fork *a*, the other to the fork *b*, which vibrates with twice the rapidity of *a*. Stretched by a weight of 20 grains, the string attached to *b* vibrates as a whole. Substituting *b* for *a*, a

FIG. 50.

80 grains.



weight of 80 grains causes the string to vibrate as a whole. Hence, to double the rapidity of vibration, we must quadruple the stretching weight. In the same way it might be proved, that to treble the rapidity of vibration we should have to make the stretching weight nine-fold. From this it follows that:—

The rapidity of vibration is proportional to the square root of the tension.

Let us vary this experiment. This silk cord is carried from the tuning-fork over the pulley, and stretched by a weight of 80 grains. The string vibrates as a whole as at A, fig. 50. By diminishing the weight the string is relaxed, and finally divides sharply into two ventral segments, as at B, fig. 50. What is now the stretching weight? 20 grains, or one-fourth of the first. With a stretching weight of almost exactly 9 grains it divides into three segments, as at C; while with a stretching weight of 5 grains it divides into four segments, as at D. Thus, then, a tension of one-fourth doubles, a tension of one-ninth trebles, and a tension of one-sixteenth quadruples the number of ventral segments. In general terms, the number of segments is inversely proportional to the square root of the tension. This result may be deduced by reasoning from the preceding laws, and its realisation here confirms their correctness.

Thus, by a series of reasonings and experiments totally different from those formerly employed, we arrive at the self-same laws. In science, different lines of reasoning often converge upon the same truth; and if we only follow them faithfully, we are sure to reach that truth at last. We may emerge, and often do emerge, from our reasoning with a contradiction in our hands; but, on retracing our steps, we infallibly find the cause of the contradiction to be due, not to any lack of uniformity in nature, but of accuracy in man.

HARMONIC SOUNDS OR OVERTONES.

§ 9. *Timbre; Klangfarbe; Clang-tint.*

We now approach a portion of our subject which will subsequently prove to be of the very highest importance. It has been shown by the most varied experiments that a stretched string can either vibrate as a whole, or divide itself into a number of equal parts, each of which vibrates as an independent string. Now, it is not possible to sound the string as a whole without at the same time causing, to a greater or less extent, its subdivision; that is to say, superposed upon the vibrations of the whole string we have always, in a greater or less degree, the vibrations of its aliquot parts. The higher notes produced by these latter vibrations are called the *harmonics* of the string. And so it is with other sounding bodies; we have in all cases a coexistence of vibrations. Higher tones mingle with the fundamental one, and it is their intermixture which determines what, for want of a better term, we call the *quality* of the sound. The French call it *timbre*, and the Germans call it *Klangfarbe*.[1] It is this union of high and low tones that enables us to distinguish one musical instrument from another. A clarionet and a violin, for example, though tuned to the same fundamental note, are not confounded; the auxiliary tones of the one are different from those of the other, and these latter tones, uniting themselves to the fundamental tones of the two instruments, destroy the identity of the sounds.

All bodies and instruments, then, employed for producing musical sounds emit, besides their fundamental tones, others due to higher orders of vibration. The Germans embrace all such sounds under the general term

[1] 'This quality of sound, sometimes called its register, colour, or timbre.'—THOMAS YOUNG, *Essay on Music*.

Obertöne. I think it will be an advantage if we in England adopt the term *overtones* as the equivalent of the term employed in Germany. One has occasion to envy the power of the German language to adapt itself to requirements of this nature. The term *Klangfarbe*, for example, employed by Helmholtz is exceedingly expressive, and we need its equivalent also. Colour depends upon rapidity of vibration, blue light bearing to red the same relation that a high tone does to a low one. A simple colour has but one rate of vibration, and it may be regarded as the analogue of a simple tone in music. A *tone*, then, may be defined as the product of a vibration which cannot be decomposed into more simple ones. A compound colour, on the contrary, is produced by the admixture of two or more simple ones; and an assemblage of tones, such as we obtain when the fundamental tone and the harmonics of a string sound together, is called by the Germans a *Klang*. May we not employ the English word *clang* to denote the same thing, and thus give the term a precise scientific meaning akin to its popular one? And may we not, like Helmholtz, add the word *colour* or *tint*, to denote the character of the clang, using the term *clang-tint* as the equivalent of Klangfarbe?

With your permission I shall henceforth employ these terms; and it now becomes our duty to look a little more closely than we have hitherto done into the subdivision of a string into its harmonic segments. Our monochord with its stretched wire is before you. The scale of the instrument is divided into 100 equal parts. At the middle point of the wire stands the number 50; at a point almost exactly one-third of its length from its end stands the number 33; while at distances equal to one-fourth and one-fifth of its length from its end stand the numbers 25 and 20 respectively. These numbers are sufficient for our present purpose. When the wire is

plucked at 50 you hear its clang, rather hollow and dull. When plucked at 33, the clang is different. When plucked at 25, the clang is different from either of the former. As we retreat from the centre of the string, the clang-tint becomes more 'brilliant,' the sound more brisk and sharp. What is the reason of these differences in the sound of the same wire?

The celebrated Thomas Young, once professor in this Institution, enables us to solve the question. He proved that when any point of a string is plucked, all the higher tones *which require that point for a node* vanish from the clang. Let me illustrate this experimentally. I pluck the point 50, and permit the string to sound. It may be proved that the first overtone, which corresponds to a division of the string into two vibrating parts, is now absent from the clang. If it were present, the damping of the point 50 would not interfere with it, for this point would be its node. But on damping the point 50 the fundamental tone is quenched, and no octave of that tone is heard. Along with the octave its whole progeny of overtones, with rates of vibration four times, six times, eight times —all even numbers of times—the rate of the fundamental tone, disappear from the clang. All these tones require that a node should exist at the centre, where, according to the principle of Young, it cannot now be formed. Let us pluck some other point, say 25, and damp 50 as before. The fundamental tone is now gone, but its octave, clear and full, rings in your ears. The point 50 in this case not being the one plucked, a node can form there; it *has* formed, and the two halves of the string continue to vibrate after the vibrations of the string as a whole have been extinguished. Plucking the point 33, the second harmonic or overtone is absent from the clang. This is proved by damping the point 33. If the second harmonic were on the string this would not

affect it, for 33 is its node. The fundamental is quenched, but no tone corresponding to a division of the string into three vibrating parts is now heard. The tone is not heard because it was never there.

All the overtones which depend on this division, those with six times, nine times, twelve times the rate of vibration of the fundamental one, are also withdrawn from the clang. Let us now pluck 20, damping 33 as before. The second harmonic is not extinguished, but continues to sound clearly and fully after the extinction of the funmental tone. In this case the point 33 not being that plucked, a node can form there, and the string can divide itself into three parts accordingly. In like manner, if 25 be plucked and then damped, the third harmonic is not heard; but when a point between 25 and the end of the wire is plucked, and the point 25 damped, the third harmonic is plainly heard. And thus we might proceed, the general rule enunciated by Young, and illustrated by these experiments, being, that when any point of a string is plucked or struck, or, as Helmholtz adds, agitated with a bow, the harmonic which requires that point for a node vanishes from the general clang of the string.

§ 10. *Mingling of Overtones with Fundamental. The Eolian Harp.*

You are now in a condition to estimate the influence which these higher vibrations must have upon the quality of the tone emitted by the string. The sounds which ring in your ears so plainly after the fundamental tone is quenched mingled with that note before it was extinguished. It seems strange that tones of such power could be so masked by the fundamental, that even the disciplined ear of a musician is unable to separate the one from the other. But Helmholtz has shown that this is due to

want of practice and attention. The musician's faculties were never exercised in this direction. There are numerous effects which the musician can distinguish, because his art demands the habit of distinguishing them. But it is no necessity of his art to resolve the clang of an instrument into its constituent tones. By attention, however, even the unaided ear can accomplish this, particularly if the mind be informed beforehand what the ear has to bend itself to find.

And this reminds me of an occurrence which took place in this room at the beginning of my acquaintance with Faraday. I wished to show him a peculiar action of an electro-magnet upon a crystal. Everything was arranged, when just before the magnet was excited he laid his hand upon my arm and asked, 'What am I to look for?' Amid the assemblage of impressions connected with an experiment, even this prince of experimenters felt the advantage of having his attention directed to the special point to be illustrated. Such help is the more needed when we attempt to resolve into its constituent parts an effect so intimately blended as the composite tones of a clang. When we desire to isolate a particular tone, one way of helping the attention is to sound that tone feebly on a string of the proper length. Thus prepared, the ear glides more readily from the single tone to that of the same pitch in a composite clang, and detaches it more readily from its companions. In the experiments executed a moment ago, where our aim in each respective case was to bring out the higher tone of the string in all its power, we entirely extinguished its fundamental tone. It may, however, be enfeebled without being destroyed. I pluck this string at 33, and lay the feather lightly for a moment on the string at 50. The fundamental tone is thereby so much lowered that its octave can make itself plainly heard. By again touching the string at 50, the fundamental tone

is lowered still more; so that now its first harmonic is more powerful than itself. You hear the sound of both, and you might have heard them in the first instance by a sufficient stretch of attention.

The harmonics of a string may be augmented or subdued within wide limits. They may, as we have seen, be masked by the fundamental tone, and they may also effectually mask it. A stroke with a hard body is favourable while a stroke with a soft body is unfavourable to their development. They depend, moreover, on the promptness with which the body striking the string retreats after striking. Thus they are influenced by the weight and elasticity of the hammers in the pianoforte. They also depend upon the place at which the shock is imparted. When, for example, a string is struck in the centre, the harmonics are less powerful than when it is struck near one end.

Helmholtz, who is equally eminent as a mathematician and as an experimental philosopher, has calculated the theoretic intensity of the harmonics developed in various ways; that is to say, the actual *vis viva* or energy of the vibration, irrespective of its effects upon the ear. A single example given by him will suffice to illustrate this subject. Calling the intensity of the fundamental tone, in each case, 100, that of the second harmonic, when the string was simply pulled aside at a point $\frac{1}{7}$th of its length from its end and then liberated, was found to be 56·1, or a little better than one-half. When the string was struck with the hammer of a pianoforte, whose contact with the string endured for $\frac{3}{7}$ths of the period of vibration of the fundamental tone, the intensity of the same tone was 9. In this case the second harmonic was nearly quenched. When, however, the duration of contact was diminished to $\frac{3}{20}$ths of the period of the fundamental, the intensity of the harmonic rose to 357; while, when the string was sharply

struck with a very hard hammer, the intensity mounted to 505, or to more than quintuple that of the fundamental tone.[1] Pianoforte manufacturers have found that the most pleasing tone is excited by the middle strings of their instruments, when the point against which the hammer strikes is from $\frac{1}{7}$th to $\frac{1}{9}$th of the length of the wire from its extremity.

Why should this be the case? Helmholtz has given the answer. Up to the tones which require these points as nodes the overtones all form chords with the fundamental; but the sixth and eighth overtones of the wire do not enter into such chords; they are dissonant tones, and hence the desirability of doing away with them. This is accomplished by making the point at which a node is required that on which the hammer falls. The possibility of the tone forming is thereby shut out, and its injurious effect is avoided.

The strings of the Eolian harp are divided into harmonic parts by a current of air passing over them. The instrument is usually placed in a window between the sash and frame, so as to leave no way open to the entrance of the air except over the strings. Sir Charles Wheatstone recommends the stretching of a first violin string at the bottom of a door which does not closely fit. When the door is shut, the current of air entering beneath sets the string in vibration, and when a fire is in the room, the vibrations are so intense that a great variety of sounds are simultaneously produced.[2] A gentleman in Basel once constructed with iron wires a large instrument which he called the weather-harp or giant-harp, and which, according to its maker, sounded as the weather changed. Its

[1] *Lehre von den Tonempfindungen*, p. 135.

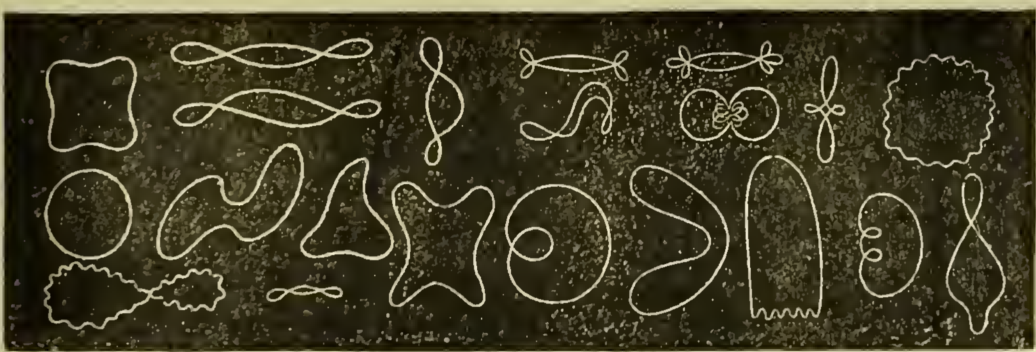
[2] The action of such a string is substantially the same as that of the syren. The string renders intermittent the current of air. Its action also resembles that of a *reed*. See Lecture V.

sounds were also said to be evoked by changes of terrestrial magnetism. Chladni pointed out the error of these notions, and reduced the action of the instrument to that of the wind upon its strings.

§ 11. *Young's Optical Illustrations.*

Finally, with regard to the vibrations of a wire, the experiments of Dr. Young, who was the first to employ optical methods in such experiments, must be mentioned. He allowed a sheet of sunlight to cross a pianoforte wire, and obtained thus a brilliant dot. Striking the wire he caused it to vibrate, the dot described a luminous line like

Fig. 51.



that produced by the whirling of a burning coal in the air, and the form of this line revealed the character of the vibration. It was rendered manifest by these experiments that the oscillations of the wire were not confined to a single plane, but that it described in its vibrations curves of greater or less complexity. Superposed upon the vibration of the whole string were partial vibrations, which revealed themselves as loops and sinuosities. Some of the lines observed by Dr. Young are given in fig. 51. Every one of these figures corresponds to a distinct impression made by the wire upon the surrounding air. The form of the sonorous wave is affected by these superposed vibrations, and thus they influence the clang-tint or quality of the sound.

SUMMARY OF LECTURE III.

The amount of motion communicated by a vibrating string to the air is too small to be perceived as sound. even at a small distance from the string.

When a broad surface vibrates in air, condensations and rarefactions are more readily formed than when the vibrating body is of small dimensions like a string. Hence, when strings are employed as sources of musical sounds, they are associated with surfaces of larger area which take up their vibrations, and transfer them to the surrounding air.

Thus the tone of a harp, a piano, a guitar, or a violin, depends mainly upon the sound-board of the instrument.

The following four laws regulate the vibrations of strings:— The rate of vibration is inversely proportional to the length; it is inversely proportional to the diameter; it is directly proportional to the square root of the stretching weight or tension; and it is inversely proportional to the square root of the density of the string.

When strings of the same length and tension but of different diameters and densities are compared, the law is that the rate of vibration is inversely proportional to the square root of the weight of the string.

When a stretched rope, or an india-rubber tube filled with sand, with one of its ends attached to a fixed object, receives a jerk at the other end, the protuberance raised upon the tube runs along it as a pulse to its fixed end, and, being there reflected, returns to the hand by which the jerk was imparted.

The time required for the pulse to travel from the hand to the fixed end of the tube and back is that required by the whole tube to execute a complete vibration.

When a series of pulses are sent in succession along the tube, the direct and reflected pulses meet, and by their coalescence divide the tube into a series of vibrating parts, called *ventral segments*, which are separated from each other by points of apparent rest called *nodes*.

The number of ventral segments is directly proportional to the rate of vibration at the free end of the tube.

The hand which produces these vibrations may move through less than an inch of space; while by the accumulation of its impulses the amplitude of the ventral segments may amount to several inches, or even to several feet.

If an india-rubber tube, fixed at both ends, be encircled at its centre by the finger and thumb, when either of its halves is pulled aside and liberated, both halves are thrown into a state of vibration.

If the tube be encircled at a point one-third, one-fourth, or one-fifth of its length from one of its ends, on pulling the shorter segment aside and liberating it, the longer segment divides itself into two, three, or four vibrating parts, separated from each other by nodes.

The number of vibrating segments depends upon the rate of vibration at the point encircled by the finger and thumb.

Here also the amplitude of vibration at the place encircled by the finger and thumb may not be more than a fraction of an inch, while the amplitude of the ventral segments may amount to several inches.

A musical string damped by a feather at a point one-half, one-third, one-fourth, one-fifth, &c., of its length from one of its ends, and having its shorter segments agi-

tated, divides itself exactly like the india-rubber tube. Its divisions may be rendered apparent by placing little paper riders across it. Those placed at the ventral segments are thrown off, while those placed at the nodes retain their places.

The notes corresponding to the division of a string into its aliquot parts are called the *harmonics* of the string.

When a string vibrates as a whole, it usually divides at the same time into its aliquot parts. Smaller vibrations are superposed upon the larger, the tones corresponding to those smaller vibrations, and which we have agreed to call overtones, mingling at the same time with the fundamental tone of the string.

The addition of these overtones to the fundamental tone determines the *timbre* or *quality* of the sound, or, as we have agreed to call it, the *clang-tint*.

It is the addition of such overtones to fundamental tones of the same pitch which enables us to distinguish the sound of a clarionet from that of a flute, and the sound of a violin from both. Could the pure fundamental tones of these instruments be detached, they would be undistinguishable from each other; but the different admixture of overtones in the different instruments renders their clang-tints diverse, and therefore distinguishable.

Instead of the heavy india-rubber tube in the experiment above referred to, we may employ light silk strings, and, instead of the vibrating hand, we may employ vibrating tuning-forks, and cause the strings to swing as a whole, or to divide themselves into any number of ventral segments. Effects of great beauty are thus obtained, and by experiments of this character all the laws of vibrating strings may be demonstrated.

When a stretched string is plucked aside or agitated

by a bow, all the overtones which require the agitated point for a node vanish from the sound of the string.

The point struck by the hammer of a piano is from one-seventh to one-ninth of the length of a string from its end: by striking this point, the notes which require it as a node cannot be produced, a source of dissonance being thus avoided

LECTURE IV.

TRANSVERSE VIBRATIONS OF A ROD FIXED AT BOTH ENDS: ITS SUBDIVISIONS AND CORRESPONDING OVERTONES—VIBRATIONS OF A ROD FIXED AT ONE END—THE KALEIDOPHONE—THE IRON FIDDLE AND MUSICAL BOX—VIBRATIONS OF A ROD FREE AT BOTH ENDS—THE CLAQUEBOIS AND GLASS HARMONICA—VIBRATIONS OF A TUNING-FORK: ITS SUBDIVISION AND OVERTONES—VIBRATIONS OF SQUARE PLATES—CHLADNI'S DISCOVERIES—WHEATSTONE'S ANALYSIS OF THE VIBRATIONS OF PLATES—CHLADNI'S FIGURES—VIBRATIONS OF DISCS AND BELLS—EXPERIMENTS OF FARADAY AND STREHLKE.

§ 1. *Transverse Vibrations of a Rod fixed at both ends.*

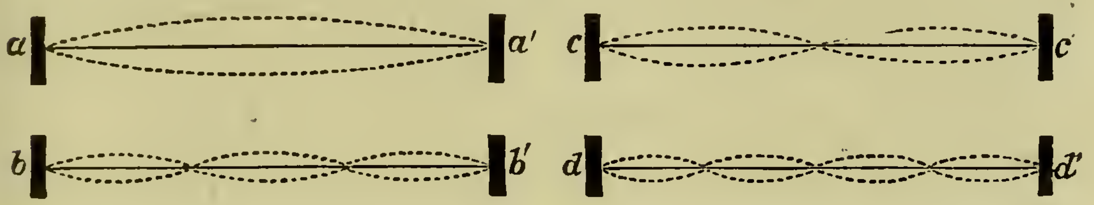
OUR last lecture was devoted to the transverse vibrations of strings. This one I propose devoting to the transverse vibrations of rods, plates, and bells, commencing with the case of a rod fixed at both ends. Its modes of vibration are exactly those of a string. It vibrates as a whole, and can also divide itself into two, three, four, or more vibrating parts. But, for a reason to be immediately assigned, the laws which regulate the pitch of the successive notes are entirely different in the two cases. Thus, when a string divides into two equal parts, each of its halves vibrates with twice the rapidity of the whole; while, in the case of the rod, each of its halves vibrates with nearly three times the rapidity of the whole. With greater strictness, the ratio of the two rates of vibration is as 9 is to 25, or as the square of 3 to the square of 5. In fig. 52, *a a′*, *c c′*, *b b′*, *d d′*, are sketched the first four modes of vibration of a rod fixed at both ends: the successive rates of vibration in the four cases bear to each other the following relation:—

Number of nodes . .	0	1	2	3
Number of vibrations .	9	25	49	81

the last row of figures being the squares of the odd numbers 3, 5, 7, 9.

In the case of a string, the vibrations are maintained by a tension externally applied; in the case of a rod, the vibrations are maintained by the elasticity of the rod itself. The modes of division are in both cases the same,

FIG. 52.



but the forces brought into play are different, and hence also the successive rates of vibration.

§ 2. *Transverse Vibrations of a Rod fixed at one end.*

Let us now pass on to the case of a rod fixed at one end and free at the other. Here also it is the elasticity of the material, and not any external tension, that sustains the vibrations. Approaching, as usual, sonorous vibrations through more grossly mechanical ones, I fix this long rod of iron, *n o*, fig. 53, in a vice, draw it aside, and liberate it. To make its vibrations more evident, its shadow is thrown upon a screen. The rod oscillates as a whole to and fro, between the points *p p′*. But it is capable of other modes of vibration. Damping it at the point *a*, by holding it gently there between the finger and thumb, and striking it sharply between *a* and *o*, the rod divides into two vibrating parts, separated by a node as shown in fig. 54. You see upon the screen a shadowy spindle between *a* and the vice below, and a shadowy fan above *a*, with a black node between the two. The division may be effected without damping *a*, by merely imparting a sufficiently sharp shock to the rod between *a* and *o*. In this case, however, besides oscillating in parts, the rod oscil-

lates as a whole, the partial oscillations being superposed upon the large one.

You notice, moreover, that the amplitude of the partial oscillations depends upon the promptness of the stroke. When the stroke is sluggish, the partial division is but feebly pronounced, the whole oscillation being most marked. But when the shock is sharp and prompt, the whole oscillation is feeble, and the partial oscillations are

Fig. 53.

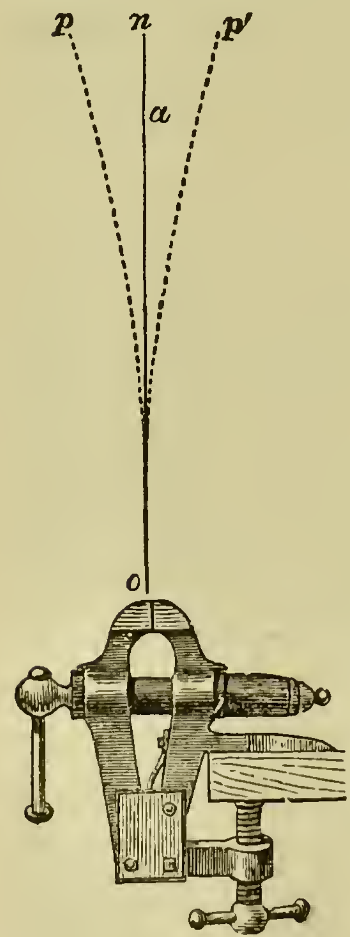


Fig. 54.

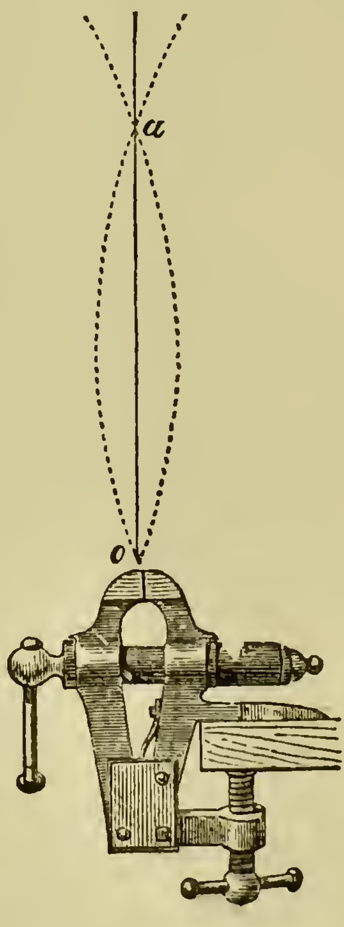
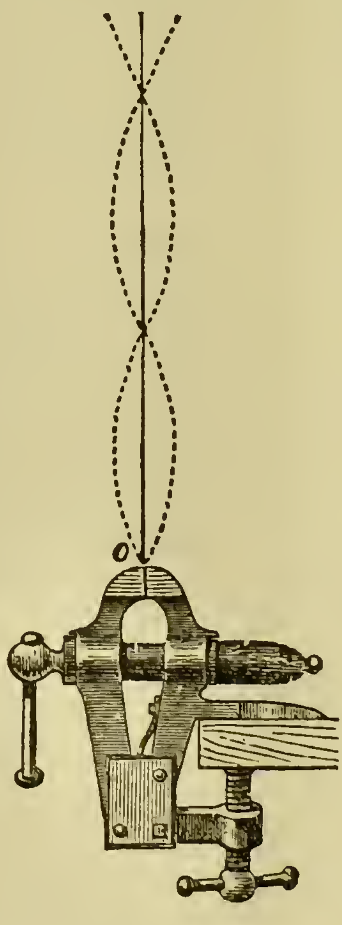


Fig. 55.



executed with vigour. If the vibrations of this rod were rapid enough to produce a musical sound, the oscillations of the rod as a whole would correspond to its fundamental tone, while the division of the rod into two vibrating parts would correspond to the first of its overtones. If, moreover, the rod vibrated as a whole and as a divided rod at the same time, the fundamental tone and the overtone

would be heard simultaneously. By damping the proper point and imparting the proper shock, we can still further subdivide the rod, as shown in fig. 55.

§ 3. *Chladni's Tonometer: the Iron Fiddle, Musical Box, and the Kaleidophone.*

And now let us shorten our rod so as to render its vibrations sonorous. When it is about four inches long it emits a low musical sound on being agitated by a bow. When further shortened, the tone is higher; and by continuing to shorten the rod, the speed of vibration is augmented, until finally the sound becomes painfully acute. These musical vibrations differ only in rapidity from the grosser oscillations which a moment ago appealed to the eye.

The increase in the rate of vibration here observed is ruled by a definite law: the number of vibrations executed in a given time is inversely proportional to the square of the length of the vibrating rod. You hear the sound of this strip of brass, two inches long, as the fiddle bow is passed over its end. Making the length of the strip one inch, the sound is the double octave of the last one; the rate of vibration is augmented four times. Thus by doubling the length of the vibrating strip we reduce its rate of vibration to one-fourth; by trebling the length we reduce the rate of vibration to one-ninth; by quadrupling the length we reduce the vibrations to one-sixteenth, and so on. It is plain that by proceeding in this way we should finally reach a length where the vibrations would be sufficiently slow to be counted. Or, beginning with a long strip whose vibrations could be counted, we might, by shortening, not only make the strip sound, but also determine the rates of vibration corresponding to its different tones. Supposing we start with a strip 36 inches long, which vibrates once in a

second: the strip reduced to 12 inches would, according to the above law, execute 9 vibrations a second; reduced to 6 inches, it would execute 36; to 3 inches, 144; while if reduced to 1 inch in length, it would execute 1,296 vibrations in a second. It is easy to fill the spaces between the lengths here given, and thus to determine the rate of vibration corresponding to any particular tone. This method was proposed and carried out by Chladni.

A musical instrument may be formed of short rods. Into this common wooden tray a number of pieces of stout iron wire of different lengths are fixed, being ranged in a semicircle. When the fiddle-bow is passed over the series, we obtain a succession of very pleasing notes. A competent performer could certainly extract very tolerable music from a sufficient number of these iron pins. The iron fiddle (*violon de fer*) is thus formed. The notes of the ordinary musical-box are also produced by the vibrations of tongues of metal fixed at one end. Pins are fixed in a revolving cylinder, the free ends of the tongues are lifted by these pins and then suddenly let go. The tongues vibrate, their length and strength being so arranged as to produce in each particular case the proper rapidity of vibration.

Sir Charles Wheatstone has devised a simple and ingenious optical method for the study of vibrating rods fixed at one end. Attaching light glass beads, silvered within, to the end of a metal rod, and allowing the light of a lamp or candle to fall upon the bead, he obtained a small and intensely illuminated spot. When the rod vibrated, this spot described a brilliant line which showed the character of the vibration. A knitting-needle fixed in a vice with a small bead stuck on to it by marine glue answers perfectly as an illustration. In Wheatstone's more complete instrument, which he calls a kaleidophone, the vibrating rods are firmly screwed into a massive stand. Extremely beautiful

figures are obtained by this simple contrivance, some of which may now be projected on a magnified scale upon the screen before you.

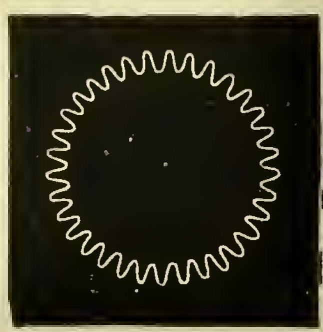
Fixing the rod horizontally in the vice, a condensed beam is permitted to fall upon the silvered bead, a spot of sunlike brilliancy being thus obtained. Placing a lens in front of the bead, a bright image of the spot is thrown upon the screen, the needle is then drawn aside, and suddenly liberated. The spot describes a ribbon of light, at first straight, but speedily opening out into an ellipse, passing into a circle, and then again through a second ellipse back to a straight line. This is due to the fact that a rod held thus in a vice vibrates not only in the direction in which it is drawn aside, but also at right angles to this direction. The curve is due to the combination of two rectangular vibrations.[1] While the rod is thus swinging as a whole, it may also divide itself into vibrating parts. By properly drawing a violin-bow across the needle this serrated circle, fig. 56, is obtained, a number of small undulations being superposed upon the large one. You moreover hear a musical tone, which you did not hear when the rod vibrated as a whole only; its oscillations, in fact, were then too slow to excite such a tone. The vibrations which produce these sinuosities, and which correspond to the first division of the rod, are executed with about $6\frac{1}{4}$ times the rapidity of the vibrations of the rod swinging as a whole. Again I draw the bow; the note rises in pitch, the serrations run more closely together, forming on the screen a luminous ripple more minute, and, if possible,

Fig. 56.

[1] Chladni also observed this compounding of vibrations, and executed a series of experiments, which, in their developed form, are those of the kaleidophone. The composition of vibrations will be studied at some length in a subsequent lecture.

more beautiful than the last one, fig. 57. Here we have the second division of the rod, the sinuosities of which correspond to $17\frac{13}{36}$ times its rate of vibration as a whole. Thus every change in the sound of the rod is accompanied by a change of the figure upon the screen.

Fig. 57.

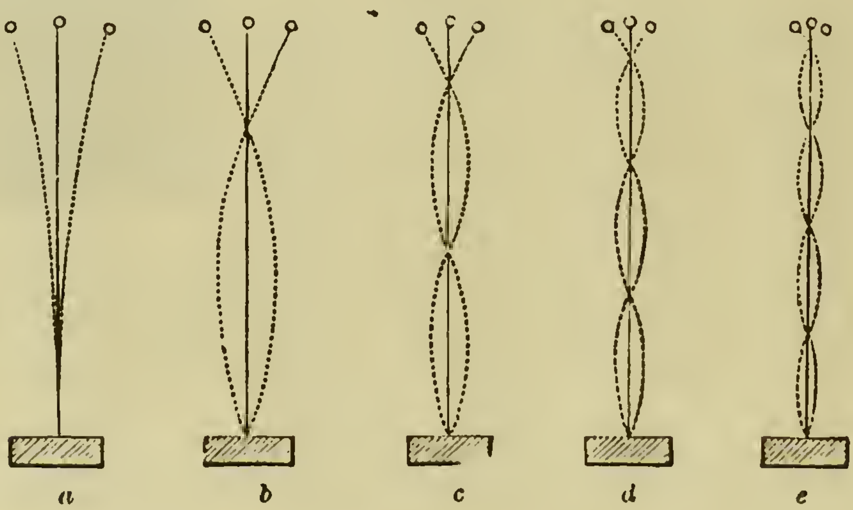


The rate of vibration of the rod as a whole, is to the rate corresponding to its first division nearly as the square of 2 is to the square of 5, or as 4 : 25. From the first division onwards the rates of vibration are approximately proportional to the squares of the series of odd numbers 3, 5, 7, 9, 11, &c. Supposing the vibrations of the rod as a whole to number 36 per unit of time, then the vibrations corresponding to this and to its successive divisions would be expressed approximately by the following series of numbers :—

36, 225, 625, 1225, 2025, &c.

In fig. 58, *a*, *b*, *c*, *d*, *e*, are shown the modes of division corresponding to this series of numbers. You

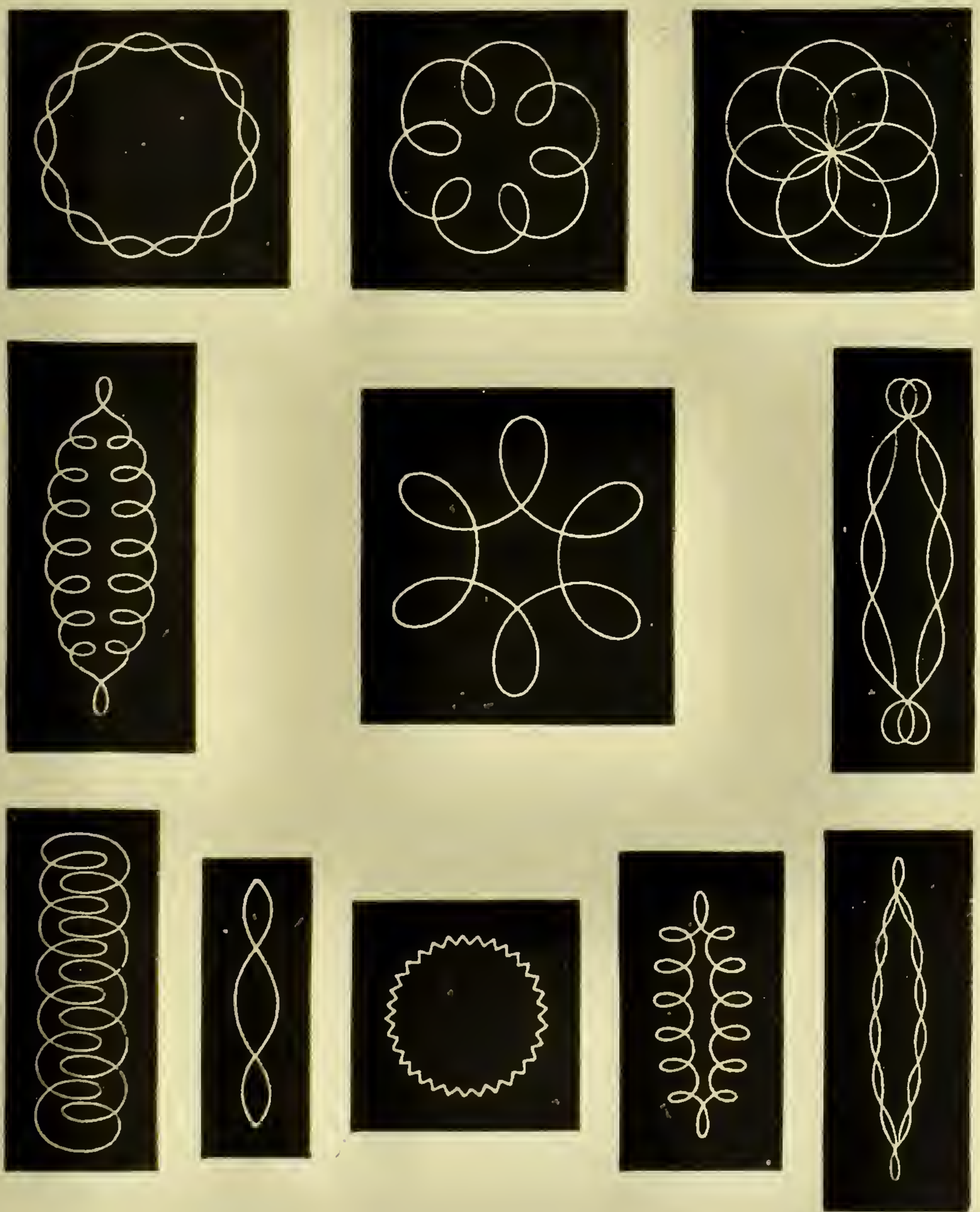
Fig. 58.



will not fail to observe that these overtones of a vibrating rod rise far more rapidly in pitch than the harmonics of a string.

Other forms of vibration may be obtained by smartly striking the rod with the finger near its fixed end. In fact, an almost infinite variety of luminous scrolls can be thus produced, the beauty of which may be inferred from

FIG. 59.



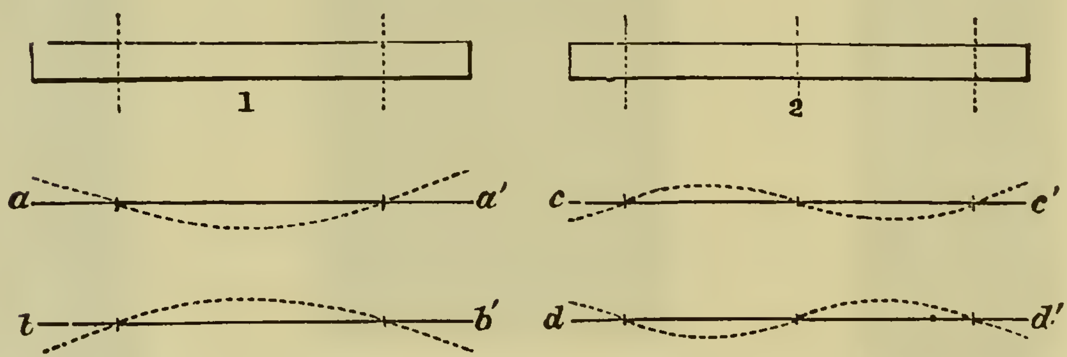
the subjoined figures first obtained by Sir C. Wheatstone. They may be produced by illuminating the bead with sunlight, or with the light of the lamp or candle. The scrolls, moreover, may be doubled by employing twc

candles instead of one. Two spots of light then appear, each of which describes its own luminous line when the knitting-needle is set in vibration. The kaleidophone was thus converted into a photometer by Sir C. Wheatstone.

§ 4. *Transverse Vibrations of a Rod free at both ends. The Claque-bois and Glass Harmonica.*

From a rod or bar fixed at one end, we will now pass to rods or bars free at both ends; for such an arrangement has also been employed in music. By a method afterwards to be described, Chladni, the father of modern acoustics, determined experimentally the modes of vibration possible to such bars. The simplest mode of division

FIG. 60.



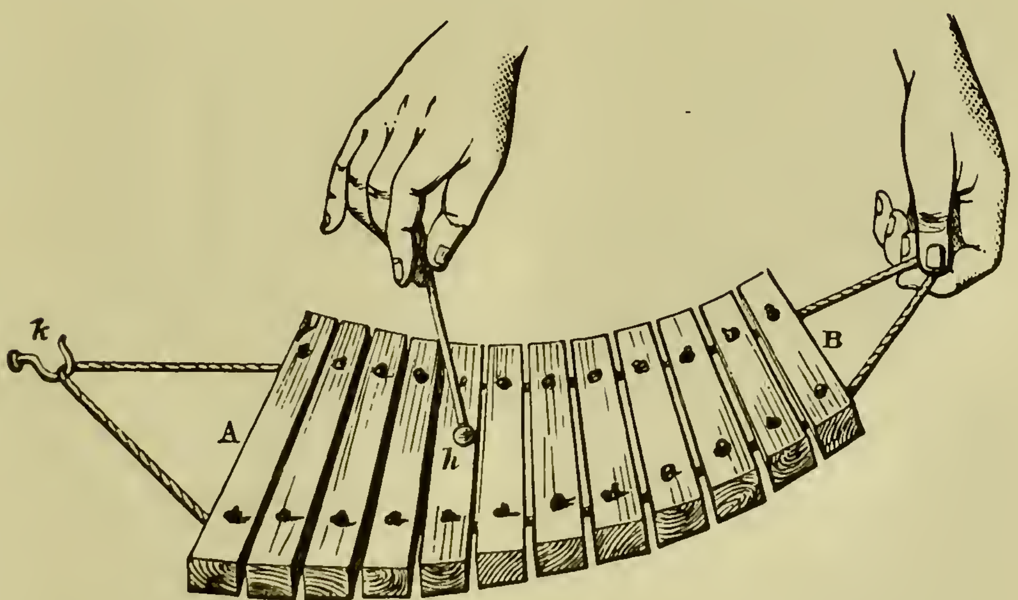
in this case occurs when the rod is divided by two nodes into three vibrating parts. This division is easily illustrated by a flexible box ruler, six feet long. Holding it at about twelve inches from its two ends between the forefinger and thumb of each hand, and shaking it, or causing its centre to be struck, it vibrates, the middle segment forming a shadowy spindle, and the two ends forming fans. The shadow of the ruler on the screen renders the mode of vibration very evident. In this case the distance of each node from the end of the ruler is about one-fourth of the distance between the two nodes. In its second mode of vibration the rod or ruler is divided into four vibrating parts by three nodes. In fig. 60, 1

and 2, these respective modes of division are shown. Looking at the edge of the ruler 1, the dotted lines cutting $a\ a'$, $b\ b'$, show the manner in which the segments bend up and down when the first division occurs, while $c\ c'$, $d\ d'$, show the mode of vibration corresponding to the second division. The deepest tone of a rod free at both ends is higher than the deepest tone of a rod fixed at one end in the proportion of 4 : 25. Beginning with the first two nodes, the rates of vibration of the free rod rise in the following proportion:—

Number of nodes	2, 3, 4, 5, 6, 7.
Numbers to the squares of which the pitch is approximately proportional	3, 5, 7, 9, 11, 13.

Here, also, we have a similarly rapid rise of pitch to that noticed in the last two cases.

Fig. 61.



For musical purposes the first division only of a free rod has been employed. When bars of wood, of different lengths, widths, and depths, are strung along a cord which passes through the nodes, we have the *claque-bois* of the French, an instrument now before you, A B, fig. 61. Supporting the cord at one end by a hook *k*, and holding it at the other in the left hand, I run the hammer, *h*,

along the series of bars, and produce an agreeable succession of musical tones. Instead of using the cord, the bars may rest at their nodes on cylinders of twisted straw; hence the name 'straw-fiddle,' sometimes applied to this instrument. Chladni informs us that it is introduced as a play of bells (Glockenspiel) into Mozart's opera of 'The Zauberflöte.' If, instead of bars of wood, we employ strips of glass, we have the glass harmonica.

§ 5. *Vibrations of a Tuning-fork.*

From the vibrations of a bar free at both ends, it is easy to pass to the vibrations of a tuning-fork, as analysed by Chladni. Supposing *a a*, fig. 62, to represent a straight

Fig. 62.

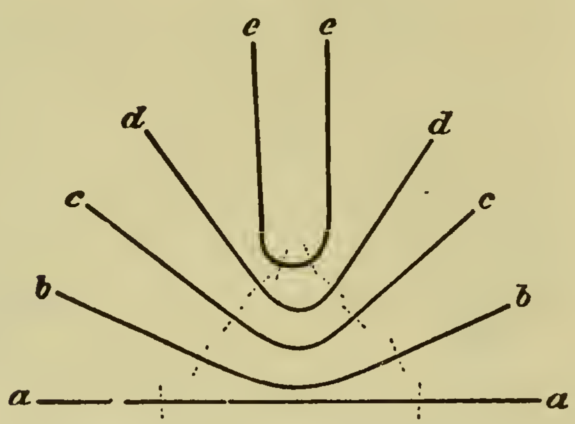


Fig. 63.

steel bar, with the nodal points corresponding to its first mode of division marked by the transverse dots. Let the bar be bent to the form *b b*; the two nodal points still remain, but they have approached nearer to each other. The tone of the bent bar is also somewhat lower than that of the straight one. Passing through various stages of bending, *c c*, *d d*, we at length convert the bar into a tuning-fork *e e*, with parallel prongs; it still retains its two nodal points, which, however, are much closer together than when the bar was straight.

When such a fork sounds its deepest note, its free ends oscillate as in fig. 63, where the prongs vibrate

between the limits b and n, and f and m, and where p and q are the nodes. There is no division of a tuning-fork corresponding to the division of a straight bar by three nodes. In its second node of division, which corresponds to the first overtone of the fork, we have a node on each prong, and two at the bottom. The principle of Young, referred to at p. 116, extends also to tuning-forks. To free the fundamental tone from an overtone, you draw your bow across the fork at a place where a node is required to form the latter. In the third mode of division there are two nodes on each prong and one at the bottom. In the fourth division there are two nodes on each prong and two at the bottom; while in the fifth division there are three nodes on each prong and one at the bottom. The first overtone of the fork requires, according to Chladni, $6\frac{1}{4}$ times the number of vibrations of the fundamental tone.

It is easy to elicit the overtones of tuning-forks. Here, for example, is our old series, vibrating respectively 256, 320, 384, and 512 times in a second. In passing from the fundamental tone to the first overtone of each, you notice that the interval is vastly greater than that between the fundamental tone and the first overtone of a stretched string. From the numbers just mentioned we pass at once to 1,600, 2,000, 2,400, and 3,200 vibrations a second. Chladni's numbers, however, though approximately correct, are not always rigidly verified by experiment. A pair of forks, for example, may have their fundamental tones in perfect unison and their overtones discordant. Two such forks are now before you. When the fundamental tones of both are sounded, the unison is perfect; but when the first overtones of both are sounded, they are not in unison. You hear rapid 'beats,' which grate upon the ear. By loading one of the forks with wax, the two overtones may be brought into unison; but now the fundamental tones

produce loud beats when sounded together. This could not occur if the first overtone of each fork was produced by a number of vibrations exactly $6\frac{1}{4}$ times the rate of its fundamental. In a series of forks examined by Helmholtz, the number of vibrations of the first overtone varied from 5·8 to 6·6 times that of the fundamental.

Starting from the first overtone, and including it, the rates of vibration of the whole series of overtones are as the squares of the numbers 3, 5, 7, 9, &c. That is to say, in the time required by the first overtone to execute 9 vibrations, the second executes 25, the third 49, the fourth 81, and so on. Thus the overtones of the fork rise with far greater rapidity than those of a string. They also vanish more speedily, and hence adulterate to a less extent the fundamental tone by their admixture.

§ 6. *Chladni's Figures.*

The device of Chladni for rendering these sonorous vibrations visible has been of immense importance to the science of acoustics. Lichtenberg had made the experiment of scattering an electrified powder over an electrified resin cake, the arrangement of the powder revealing the electric condition of the surface. This experiment suggested to Chladni the idea of rendering sonorous vibrations visible by means of sand strewn upon the surface of the vibrating body. Chladni's own account of his discovery is of sufficient interest to justify its introduction here.

'As an admirer of music, the elements of which I had begun to learn rather late, that is, in my nineteenth year, I noticed that the science of acoustics was more neglected than most other portions of physics. This excited in me the desire to make good the defect, and by new discovery to render some service to this part of science. In 1785 I had observed that a plate of glass or metal gave different sounds when it was struck at different places, but I could

Chladni.

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Chladni.

nowhere find any information regarding the corresponding modes of vibration. At this time there appeared in the journals some notices of an instrument made in Italy by the Abbé Mazzocchi, consisting of bells, to which one or two violin-bows were applied. This suggested to me the idea of employing a violin-bow to examine the vibrations of different sonorous bodies. When I applied the bow to a round plate of glass fixed at its middle it gave different sounds, which, compared with each other, were (as regards the number of their vibrations) equal to the squares of 2, 3, 4, 5, &c.; but the nature of the motions to which these sounds corresponded, and the means of producing each of them at will, were yet unknown to me. The experiments on the electric figures formed on a plate of resin, discovered and published by Lichtenberg, in the memoirs of the Royal Society of Göttingen, made me presume that the different vibratory motions of a sonorous plate might also present different appearances, if a little sand or some other similar substance were spread on the surface. On employing this means, the first figure that presented itself to my eyes upon the circular plate already mentioned resembled a star with ten or twelve rays, and the very acute sound, in the series alluded to, was that which agreed with the square of the number of diametrical lines.'

§ 7. *Vibrations of Square Plates: nodal lines.*

I will now illustrate the experiments of Chladni, commencing with a square plate of glass held by a suitable clamp at its centre. The plate might be held with the finger and thumb, if they could only reach far enough. Scattering fine sand over the plate, the middle point of one of its edges is damped by touching it with the finger nail, and a bow is drawn across the edge of the plate, near one of its corners. The sand is tossed away from certain parts of the surface, and collects along two *nodal lines*

which divide the large square into four smaller ones, as in fig. 64. This division of the plate corresponds to its deepest tone.

The signs + and — employed in these figures denote that the two squares on which they occur are always moving in opposite directions. When the squares marked + are above the average level of the plate, those marked — are below it; and when those marked — are above the average level, those marked + are below it. The nodal lines mark the boundaries of these opposing motions. They are the places of transition from the one motion to the other, and are therefore unaffected by either.

Fig. 64.

Fig. 65.

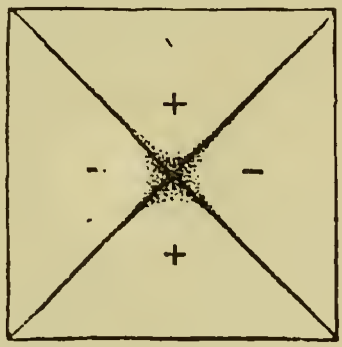
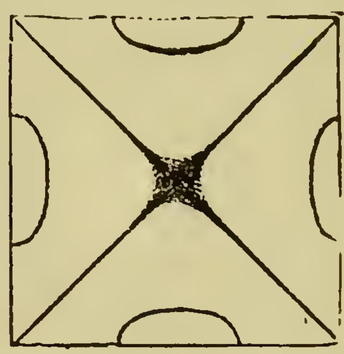


Fig. 66.



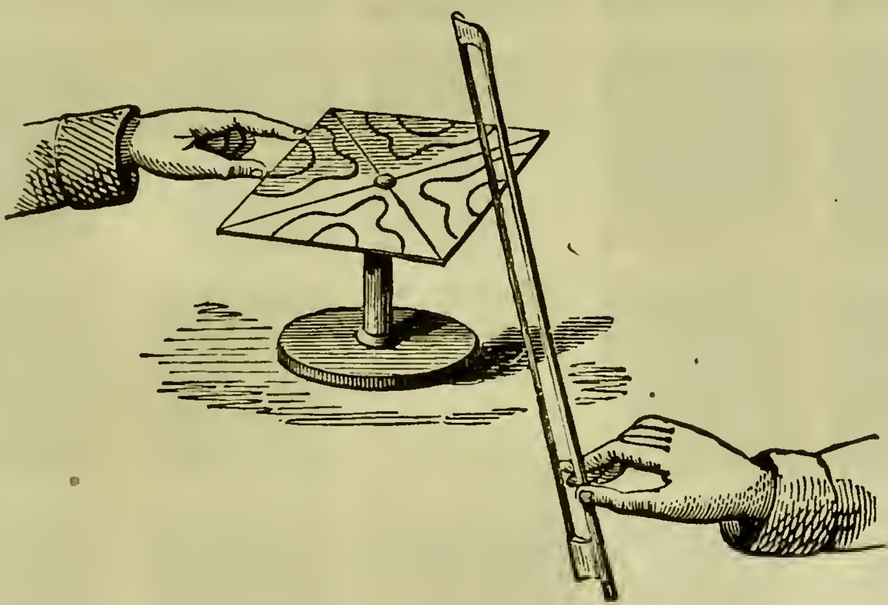
Scattering sand once more over its surface, I damp one of the corners of the plate, and excite it by drawing the bow across the middle of one of its sides. The sand dances over the surface, and finally ranges itself in two sharply-defined ridges along its diagonals, fig. 65. The note here produced is a fifth above the last. Again damping two other points, and drawing the bow across the centre of the opposite side of the plate, we obtain a far shriller note than in either of the former cases, and the manner in which the plate vibrates to produce this note is represented in fig. 66.

Thus far plates of glass have been employed held by a clamp at the centre. Plates of metal are still more suitable for such experiments. Here is a plate of brass, 12 inches square, and supported on a suitable stand. Damping it

with the finger and thumb of my left hand at two points of its edge, and drawing the bow with my right across a vibrating portion of the opposite edge, the complicated pattern represented in fig. 67 is obtained.

The beautiful series of patterns shown on page 142 were obtained by Chladni, by damping and exciting square plates in different ways. It is not only interesting but startling to see the suddenness with which these sharply

Fig. 67.

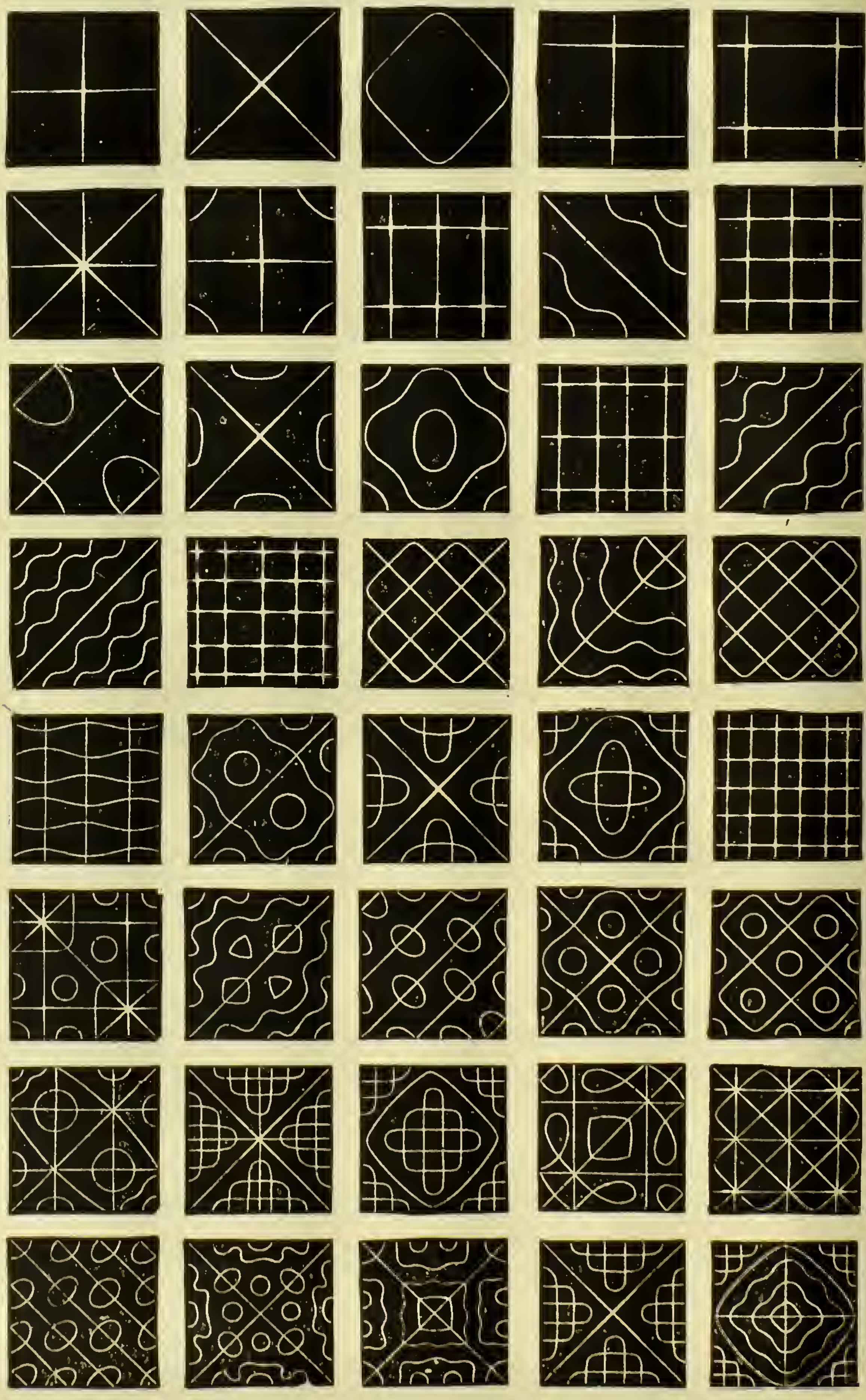


defined figures are formed by the sweep of the bow of a skilful experimenter.

§ 8. *Wheatstone's Analysis of the Vibrations of Square Plates.*

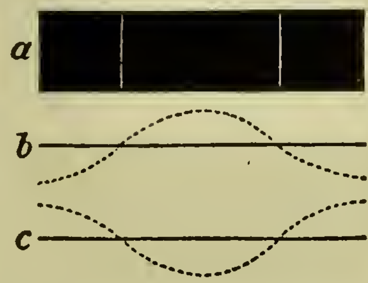
And now let us look a little more closely into the mechanism of these vibrations. The manner in which a bar free at both ends divides itself when it vibrates transversely has been already explained. Rectangular pieces of glass or of sheet metal—the glass strips of the harmonica, for example—obey the laws of free rods and bars. In fig. 69 (p. 143) is drawn a rectangle *a*, with the nodes corresponding to its first division marked upon it, and underneath it is placed a figure showing the manner in

FIG. 68.



which the rectangle, looked at edgeways, bends up and down when it is set in vibration.[1] For the sake of plainness the bending is greatly exaggerated. The figures *b* and *c* indicate that the vibrating parts of the plate alternately rise above and fall below the average level of the plate. At one moment, for example, the centre of the plate is above the level and its ends below it, as at *b*; while at the next moment its centre is below and its two ends above the average level, as at *c*. The vibrations of the plate consist in the quick successive assumption of these two positions. Similar remarks apply to all other modes of division.

FIG. 69.



Now suppose the rectangle gradually to widen, till it becomes a square. There then would be no reason why the nodal lines should form parallel to one pair of sides rather than to the other. Let us now examine what would be the effect of the coalescence in the same plate of two such systems of vibrations.

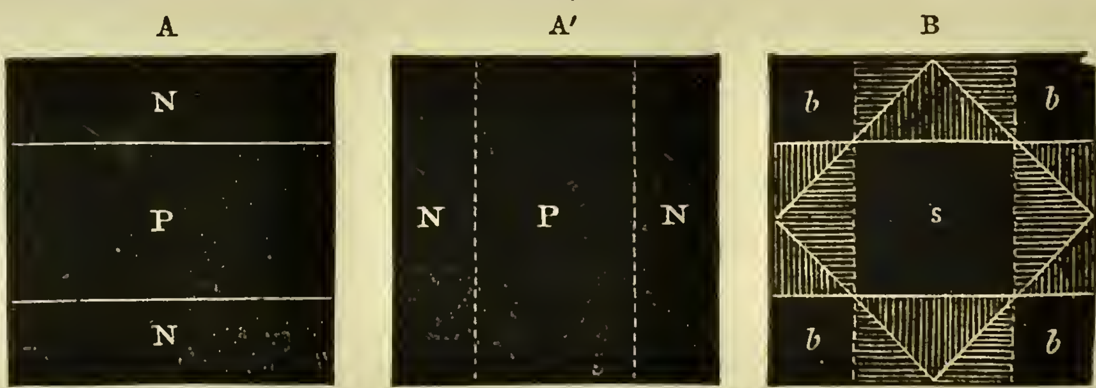
To keep your conceptions clear, take two squares of glass and draw upon each of them the nodal lines belonging to a rectangle. Draw the lines on one plate in white and on the other in black; this will help you to keep the plates distinct in your mind as you look at them. Now lay one square upon the other so that their nodal lines shall coincide, and then realise with perfect mental clearness both plates in a state of vibration. Let us assume, in the first instance, that the vibrations of the two plates are concurrent; that the middle segment and the end segments of each rise and fall together; and now suppose

[1] I copy this figure from Sir C. Wheatstone's memoir. The nodes, however, ought to be nearer the ends, and the free terminal portions of the dotted lines ought not to show any contrary flexure. The nodal lines in the next two figures are also drawn too far from the edge of the plates. See Fig. 60.

the vibrations of one plate transferred to the other. What would be the result? Evidently vibrations of a double amplitude on the part of the plate which has received this accession. But suppose the vibrations of the two plates, instead of being concurrent, to be in exact opposition to each other that when the middle segment of the one rises the middle segment of the other falls—what would be the consequence of adding them together? Evidently a neutralization of all vibration.

Instead of placing the plates so that their nodal lines coincide, set these lines at right angles to each other. That is to say, push A over A′, fig. 70. In these figures the

Fig. 70.



letter P means positive, indicating, in the section where it occurs, a motion of the plate upwards; while N means negative, indicating, where it occurs, a motion downwards. You have now before you a kind of check pattern, as shown in the third square, consisting of a square *s* in the middle, a smaller square *b* at each corner, and four rectangles (shaded) at the middle portions of the four sides. Let the plates vibrate, and then suppose the vibrations of one of them transferred to the other. What must result? A moment's reflection will show you that the big middle square *s* will vibrate with augmented energy, because here the vibrations of the two plates support each other. The same is true of the four smaller squares *b*, *b*, *b*, *b*, at the four corners; but you will at once convince yourselves

that the vibrations in the four shaded rectangles are in opposition, and that where their amplitudes are equal they will destroy each other. The middle point of each side of the plate of glass will, therefore, be a point of rest; the points where the nodal lines of the two plates cross each other will also obviously be points of rest. Draw a line through every three of these points and you will obtain a second square inscribed in the first. The sides of this square are lines of no motion.

We have thus far been theorising. Let us now clip a square plate of glass at a point near the centre of one of its edges, and draw the bow across the adjacent corner of the plate. When the glass is homogeneous, a close approximation to this inscribed square is obtained. The reason is that when the plate is agitated in this manner the two sets of vibrations which we have been considering actually co-exist in the plate, and produce the figure B fig. 70 due to their combination.

Fig. 71.

A A′

Again, place the squares of glass one upon the other exactly as in the last case; but now, instead of supposing them to concur in their vibrations, let their corresponding sections oppose each other: that is, let A cover A′, fig. 71. Then it is manifest that on superposing the vibrations the middle point of our middle square must be a point of rest; for here the vibrations are equal and opposite. The intersections of the nodal lines are also points of rest, and

so also is every corner of the plate itself, for here the vibrations are also equal and opposite. We have thus fixed four points of rest on each diagonal of the square. Draw the diagonals, and they will represent the nodal lines consequent on the superposition of the two sets of vibrations.

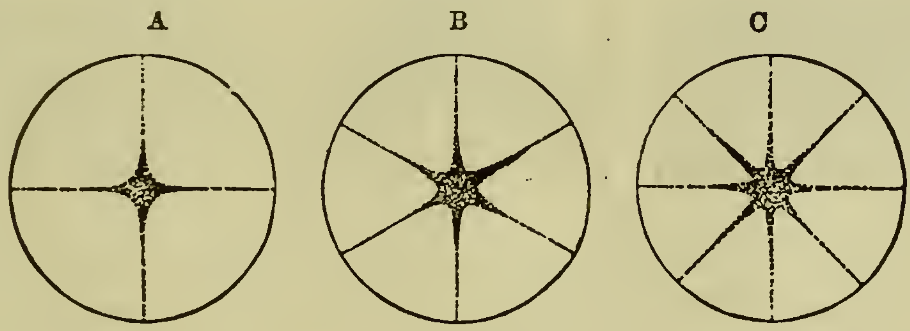
These two systems actually co-exist in the same plate when the centre is clamped and one of the corners touched, while the fiddle-bow is drawn across the middle of one of the sides. In this case the sand which marks the lines of rest arranges itself along the diagonals. This, in its simplest possible form, is Sir C. Wheatstone's analysis of these superposed vibrations.

§ 9. *Vibrations of Circular Plates.*

Passing from square plates to round ones, we also obtain various beautiful effects. This disc of brass is supported horizontally upon an upright stand: it is blackened, and fine white sand is scattered lightly over it. The disc is capable of dividing itself in various ways, and of emitting notes of various pitch. I sound the lowest fundamental note of the disc by touching its edge at a certain point, and drawing the bow across the edge at a point 45 degrees distant from the damped one. You hear the note and you see the sand. It quits the four quadrants of the disc, and ranges itself along two of the diameters, fig. 72 A (next page). When a disc divides itself thus into four vibrating segments, it sounds its deepest note. I stop the vibration, clear the disc, and once more scatter sand over it. Damping its edge, and drawing the bow across it at a point 30 degrees distant from the damped one, the sand immediately arranges itself in a star. We have here six vibrating segments, separated from each other by their appropriate nodal lines, fig. 72 B. Again I damp a point, and agitate another nearer to the damped one than in the

last instance; the disc divides itself into eight vibrating segments with lines of sand between them, fig. 72 C. In this way the disc may be subdivided into ten, twelve, fourteen, sixteen sectors, the number of sectors being always an *even* one. As the division becomes more minute the vibrations become more rapid, and the pitch consequently more high. The note emitted by the sixteen segments into which the disc is now divided is so acute as to be almost painful to the ear. Here you have Chladni's first discovery. You can understand his emotion on witnessing this wonderful effect, 'which no mortal had

FIG. 72.



previously seen.' By rendering the centre of the disc free, and damping appropriate points of the surface, nodal circles and other curved lines may be obtained.

The rate of vibration of a disc is directly proportional to its thickness, and inversely proportional to the square of its diameter. Of the three discs now before you two have the same diameter, but one is twice as thick as the other; two of them are of the same thickness, but one has half the diameter of the other. According to the law just enunciated, the rates of vibration of the discs are as the numbers 1, 2, 4. When they are sounded in succession, the musical ears present can testify that they really stand to each other in the relation of a note, its octave, and its double octave.

§ 10. *Strehlke and Faraday's Experiments: deportment of light powders.*

The actual movement of the sand towards the nodal lines may be studied by clogging the sand with a semifluid substance. When gum is employed to retard the motion of the particles, the curves which they individually de-

Fig. 73.

A B C

scribe are very clearly drawn upon the plates. M. Strehlke has sketched these appearances, and from him the patterns A, B, C, fig. 73, are borrowed.

An effect of vibrating plates which long perplexed experimenters is here to be noticed. When, with the sand

Fig. 74.

Fig. 75.

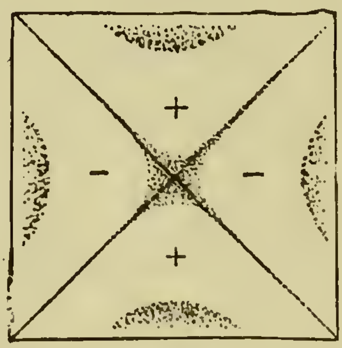
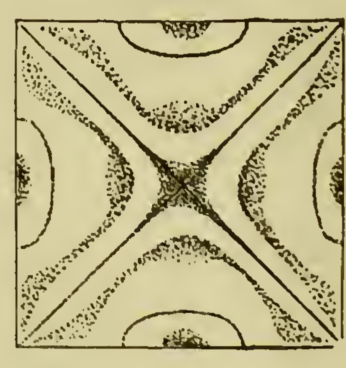


Fig. 76.



strewn over a plate, a little fine dust is mingled, say the fine seed of lycopodium; this light substance, instead of collecting along the nodal lines, forms heaps at the places of most violent motion. It is heaped at the four corners of the plate, fig. 74, at the four sides of the plate, fig. 75, and lodged between the nodal lines of the plate,

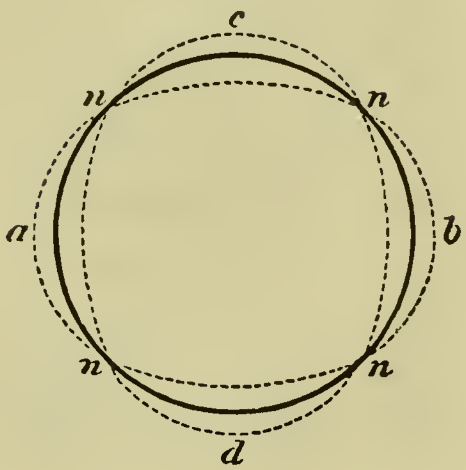
fig. 76. These three figures represent the three states of vibration illustrated in figs. 64, 65, and 66. The dust chooses in all cases the place of greatest agitation. Various explanations of this effect had been given, but it was reserved for Faraday to assign its extremely simple cause. The light powder is entangled by the little whirlwinds of air produced by the vibrations of the plate: it cannot escape from the little cyclones, though the heavier sand is readily driven through them. When, therefore, the motion ceases, the light powder settles down at the places where the vibration was a maximum. In vacuo no such effect is observed: here all powders, light and heavy, move to the nodal lines.

§ 11. *Vibration of Bells: means of rendering them visible.*

The vibrating segments and nodes of a bell are similar to those of a disc. When a bell sounds its deepest note, the coalescence of its pulses causes it to divide into four vibrating segments, separated from each other by four nodal lines, which run up from the sound-bow to the crown of the bell. The place where the hammer strikes is always the middle of a vibrating segment; the point diametrically opposite is also the middle of such a segment. Ninety degrees from these points, we have also vibrating segments, while at 45 degrees right and left of them we come upon the nodal lines. Supposing the strong dark circle in fig. 77 (next page) to represent the circumference of the bell in a state of quiescence, then when the hammer falls on any one of the segments a, c, b, or d, the sound-bow passes periodically through the changes indicated by the dotted lines. At one moment it is an oval, with $a\ b$ for its longest diameter; at the next moment it is an oval, with $c\ d$ for its longest diameter. The changes from one oval to the other, con-

stitute, in fact, the vibrations of the bell. The four points n, n, n, n, where the two ovals intersect each other, are the nodes. As in the case of a disc, the number of vibrations executed by a bell in a given time, varies directly as the thickness, and inversely as the square of the bell's diameter.

Fig. 77.



Like a disc, also, a bell can divide itself into any even number of vibrating segments, but not into an odd number. By damping proper points in succession, the bell can be caused to divide into 6, 8, 10 and 12 vibrating parts. Beginning with the fundamental note, the number of vibrations corresponding to the respective divisions of a bell, as of a disc, is as follows:—

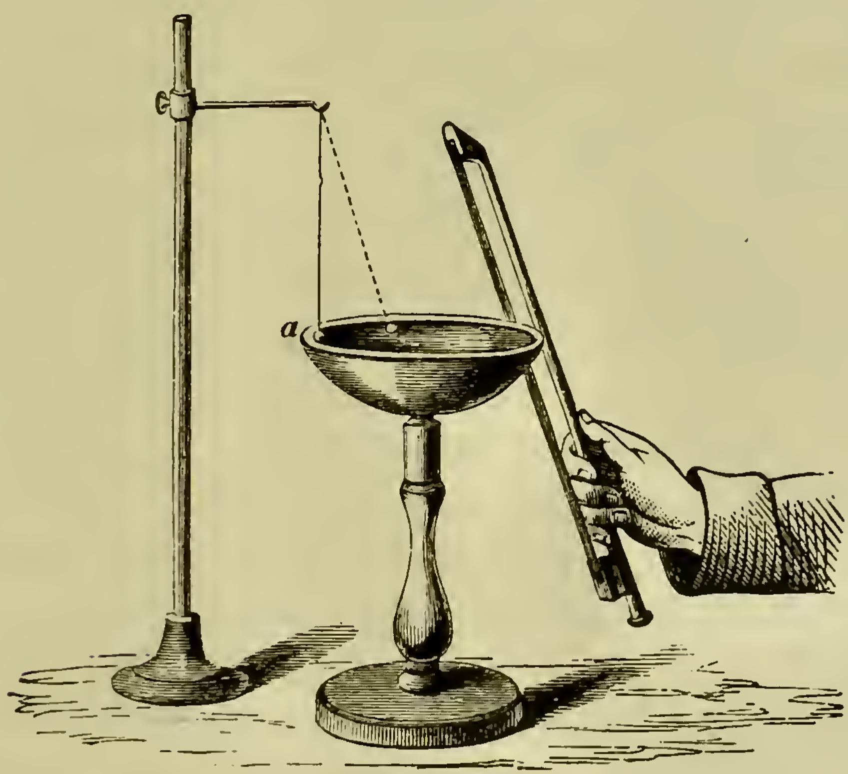
Number of divisions . . .	4, 6, 8, 10, 12.
Numbers the squares of which express the rates of vibration	2, 3, 4, 5, 6.

Thus, if the vibrations of the fundamental tone be 40, that of the next higher tone will be 90, the next 160, the next 250, the next 360, and so on. If the bell be thin, the tendency to subdivision is so great, that it is almost impossible to bring out the pure fundamental tone without the admixture of the higher ones.

I will now repeat before you a homely, but an instructive experiment. This common jug, when a fiddle-bow is drawn across its edge, divides into four vibrating segments exactly like a bell. The jug is provided with a handle; and you are to notice the influence of this handle upon the tone. When the fiddle-bow is drawn across the edge at a point diametrically opposite to the handle a certain note is heard. When it is drawn at a point 90° from the handle,

the same note is heard. In both these cases the handle occupies the middle of a vibrating segment, loading that segment by its weight. But I now draw the bow at an angular distance of 45° from the handle; the note is sensibly higher than before. The handle in this experiment occupies a node; it no longer loads a vibrating segment, and hence the elastic force, having to cope with less weight, produces a more rapid vibration. Chladni exe-

FIG. 78.



cuted with a tea-cup the experiment here made with a jug. Now, bells often exhibit round their sound-bows a lack of uniform thickness, tantamount to the want of symmetry in the case of our jug; and we shall learn subsequently, that the intermittent sound of many bells, noticed more particularly when their tones are dying out, is produced by the combination of two distinct rates of vibration, which have this absence of uniformity for their origin.

There are no points of absolute rest in a vibrating bell, for the nodes of the higher tones are not those of the

fundamental one. But it is easy to show that the various parts of the sound-bow, when the fundamental tone is predominant, vibrate with very different degrees of intensity. Suspending a little ball of sealing-wax *a*, fig. 78 (previous page), by a string, and allowing it to rest gently against the interior surface of an inverted bell, it is tossed to and fro when the bell is thrown into vibration. But the rattling of the sealing-wax ball is far more violent when it rests against the vibrating segments than when it rests against the nodes. Permitting the ivory bob of a short pendulum to rest, in succession, against a vibrating segment and against a node of the Great Bell of Westminster, I found that in the former position it was driven away five inches, in the latter only two inches and three-quarters when the hammer fell upon the bell.

Could the 'Great Bell' be turned upside down and filled with water, on striking it the vibrations would express themselves in beautiful ripples upon the liquid surface. Similar ripples may be obtained with smaller bells, or even with finger and claret glasses, but they would be too minute for our present purpose. Filling a large hemispherical glass with water, and passing the fiddle-bow across its edge; large crispations immediately cover its surface. When the bow is vigorously drawn, the water rises in spray from the four vibrating segments. Projecting, by means of a lens, a magnified image of the illuminated water surface upon the screen, I pass the bow gently across the edge of the glass, or rub my finger gently along the edge. You hear this low sound, and at the same time observe the ripples breaking as it were in visible music over the four sectors of the figure.

The experiment of Leidenfrost proves that if water be poured into a red-hot silver basin, it rolls about upon its own vapour. The same effect is produced if we drop a volatile liquid, like ether, on the surface of warm water.

And if a bell-glass be filled with ether or with alcohol, a sharp sweep of the bow over the edge of the glass detaches the liquid spherules, which, when they fall back, do not mix with the liquid, but are driven over the surface on wheels of vapour to the nodal lines. The warming of the liquid, as might be expected, improves the effect. M. Melde, to whom we are indebted for this beautiful experiment, has given the drawings, figs. 79 and 80, representing what occurs when the surface is divided

Fig. 79.

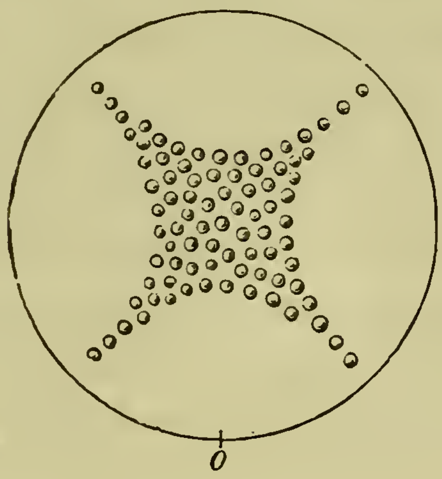
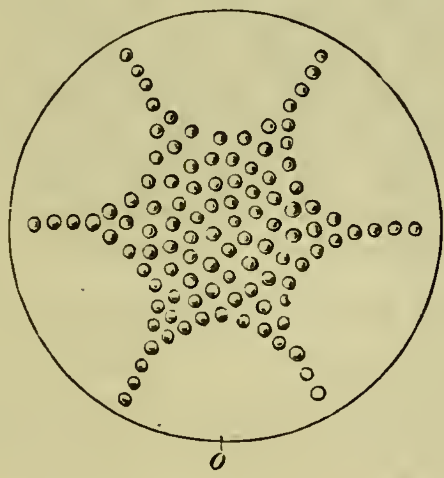


Fig. 80.



into four and into six vibrating parts. With a thin wine-glass and strong brandy the effect may also be obtained.[1]

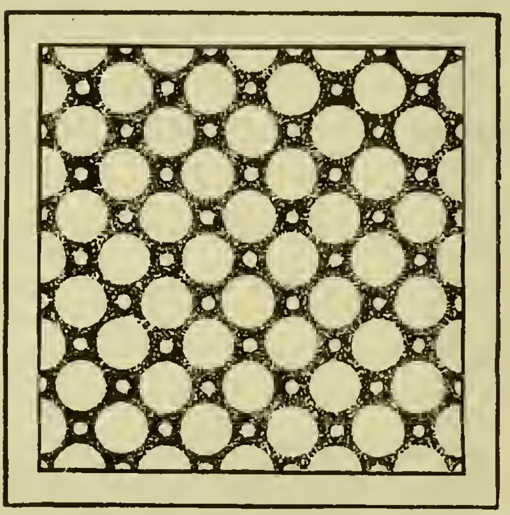
The glass and the liquid within it vibrate here together, and everything that interferes with the perfect continuity of the entire mass disturbs the sonorous effect. A crack in the glass passing from the edge downwards extinguishes its sounding power. A rupture in the continuity of the liquid has the same effect. When a glass containing a solution of carbonate of soda is struck with a bit of wood, you hear a clear musical sound. But when a little tartaric acid is added to the liquid, it foams, and a dry

[1] Under the shoulder of the Wetterhorn I found in 1867 a pool of clear water, into which a driblet fell from a brow of overhanging limestone rock. The rebounding water-drops when they fell back rolled in myriads over the surface. Almost any fountain, the spray of which falls into a basin, will exhibit the same effect.

unmusical collision takes the place of the musical sound. As the foam disappears the sonorous power returns, and now that the liquid is once more clear, you hear the musical ring as before.

The ripples of the tide leave their impressions upon the sand over which they pass. The ripples produced by sonorous vibrations have been proved by Faraday competent to do the same. Attaching a plate of glass to a long flexible board, and pouring a thin layer of water over the surface of the glass, on causing the board to vibrate, its tremors chase the water into a beautiful mosaic of ripples. A thin stratum of sand strewn upon the plate is acted upon by the water, and carved into patterns, of which fig. 81 is a reduced specimen.

Fig. 81.



SUMMARY OF LECTURE IV.

A rod fixed at both ends and caused to vibrate transversely divides itself in the same manner as a string vibrating transversely.

But the succession of its overtones is not the same as those of a string, for while the series of tones emitted by the string is expressed by the natural numbers, 1, 2, 3, 4, 5, &c.; the series of tones emitted by the rod is expressed by the squares of the odd numbers, 3, 5, 7, 9, &c.

A rod fixed at one end can also vibrate as a whole, or can divide itself into vibrating segments separated from each other by nodes.

In this case the rate of vibration of the fundamental tone is to that of the first overtone as 4 : 25, or as the square of 2 to the square of 5. From the first division onwards the rates of vibration are proportional to the squares of the odd numbers 3, 5, 7, 9, &c.

With rods of different lengths the rate of vibration is inversely proportional to the square of the length of the rod.

Attaching a glass bead silvered within to the free end of the rod, and illuminating the bead, the spot of light reflected from it describes curves of various forms when the rod vibrates. The Kaleidophone of Wheatstone is thus constructed.

The iron fiddle and the musical box are instruments whose tones are produced by rods, or tongues, fixed at one end and free at the other.

A rod free at both ends can also be rendered a source

of sonorous vibrations. In its simplest mode of division it has two nodes, the subsequent overtones correspond to divisions by 3, 4, 5, &c., nodes. Beginning with its first mode of division, the tones of such a rod are represented by the squares of the odd numbers 3, 5, 7, 9, &c.

The claque-bois, straw-fiddle, and glass harmonica are instruments whose tones are those of rods or bars free at both ends, and supported at their nodes.

When a straight bar, free at both ends, is gradually bent at its centre, the two nodes corresponding to its fundamental tone gradually approach each other. It finally assumes the shape of the tuning-fork which, when it sounds its fundamental note, is divided by two nodes near the base of its two prongs into three vibrating parts.

There is no division of a tuning-fork by three nodes.

In its second mode of division, which corresponds to the first overtone of the fork, there is a node on each prong and two others at the bottom of the fork.

The fundamental tone of the fork is to its first overtone approximately as the square of 2 is to the square of 5. The vibrations of the first overtone are, therefore, about $6\frac{1}{4}$ times as rapid as those of the fundamental. From the first overtone onwards the successive rates of vibration are as the squares of the odd numbers 3, 5, 7, 9, &c.

We are indebted to Chladni for the experimental investigation of all these points. He was enabled to conduct his inquiries by means of the discovery that, when sand is scattered over a vibrating surface, it is driven from the vibrating portions of the surface, and collects along the nodal lines.

Chladni embraced in his investigations plates of various forms. A square plate, for example, clamped at the centre, and caused to emit its fundamental tone, divides itself into four smaller squares by lines parallel to its sides.

The same plate can divide itself into four triangular vibrating parts, the nodal lines coinciding with the diagonals of the square. The note produced in this case is a fifth above the fundamental note of the plate.

The plate may be further subdivided, sand-figures of extreme beauty being produced; the notes rise in pitch as the subdivision of the plate becomes more minute.

These figures may be deduced from the coalescence of different systems of vibration.

When a circular plate clamped at its centre sounds its fundamental tone, it divides into four vibrating parts, separated by four radial nodal lines.

The next note of the plate corresponds to a division into six vibrating sectors, the next note to a division into eight sectors; such a plate can divide into any even number of vibrating sectors, the sand-figures assuming beautiful stellar forms.

The rates of vibration corresponding to the divisions of a disc are represented by the squares of the numbers 2, 3, 4, 5, 6, &c. In other words, the rates of vibration are proportional to the squares of the numbers representing the sectors into which the disc is divided.

When a bell sounds its deepest note it is divided into four vibrating parts separated from each other by nodal lines, which run upwards from the sound-bow and cross each other at the crown.

It is capable of the same subdivisions as a disc: the succession of its tones being also the same.

The rate of vibration of a disc, or bell, is directly proportional to the thickness, and inversely proportional to the square of the diameter.

LECTURE V.

LONGITUDINAL VIBRATIONS OF A WIRE—RELATIVE VELOCITIES OF SOUND IN BRASS AND IRON—LONGITUDINAL VIBRATIONS OF RODS FIXED AT ONE END—OF RODS FREE AT BOTH ENDS—DIVISIONS AND OVERTONES OF RODS VIBRATING LONGITUDINALLY—EXAMINATION OF VIBRATING BARS BY POLARISED LIGHT—DETERMINATION OF VELOCITY OF SOUND IN SOLIDS—RESONANCE—VIBRATIONS OF STOPPED PIPES: THEIR DIVISIONS AND OVERTONES—RELATION OF THE TONES OF STOPPED PIPES TO THOSE OF OPEN PIPES—CONDITION OF COLUMN OF AIR WITHIN A SOUNDING ORGAN-PIPE—REEDS AND REED-PIPES—THE VOICE—OVERTONES OF THE VOCAL CHORDS—THE VOWEL SOUNDS—THE TELEPHONE, MICROPHONE, AND PHONOGRAPH—KUNDT'S EXPERIMENTS—NEW METHODS OF DETERMINING THE VELOCITY OF SOUND.

§ 1. *Longitudinal Vibrations of Wires and Rods: conversion of Longitudinal into Transverse Vibrations.*

WE have thus far occupied ourselves exclusively with transversal vibrations: that is to say, vibrations executed at right angles to the lengths of the strings, rods, plates, and bells subjected to examination. A string is also capable of vibrating in the direction of its length, but here the power which enables it to vibrate is not a tension applied externally, but the elastic force of its own molecules. Now, this molecular elasticity is much greater than any that we can ordinarily develop by stretching the string, and the consequence is that the sounds produced by the *longitudinal vibrations* of a string are, as a general rule, much more acute than those produced by its transverse vibrations. These longitudinal vibrations may be excited by the oblique passage of a fiddle-bow; but they are more easily produced by passing briskly along the string a bit of cloth, or leather, on which powdered

resin has been strewn. The resined fingers answer the same purpose.

When the wire of our monochord is plucked aside, you hear the sound produced by its transverse vibrations. When resined leather is rubbed along the wire, a note much more piercing than the last is heard. This is due to the longitudinal vibrations of the wire. Behind the table is stretched a stout iron wire, 23 feet long. One end of it is firmly attached to an immovable wooden tray, the other end is coiled round a pin fixed firmly into one of our benches. With a key this pin can be turned, and the wire stretched so as to facilitate the passage of the rubber. Clasping the wire with the resined leather, and passing the hand to and fro along it; a rich loud musical sound is heard. Halving the wire at its centre, and rubbing one of its halves, the note heard is the octave of the last; the vibrations are twice as rapid. When the wire is clipped at one-third of its length and the shorter segment rubbed, the note is a fifth above the octave. Taking one-fourth of its length and rubbing as before, the note yielded is the double octave of that of the whole wire, being produced by four times the number of vibrations. Thus, in longitudinal as well as in transverse vibrations, the number of vibrations executed in a given time *is inversely proportional to the length of the wire.*

And notice the surprising power of these sounds when the wire is rubbed vigorously. With a shorter length, the note is so acute, and at the same time so powerful, as to be hardly bearable. It is not the wire itself which produces this intense sound; it is the wooden tray at its end, to which its vibrations are communicated. And the vibrations of the wire being longitudinal, those of the tray, which is at right angles to the wire, must be transversal. We have here, indeed, an instructive example of the conversion of longitudinal into transverse vibrations.

§ 2. *Longitudinal Pulses in Iron and Brass: their relative velocities determined.*

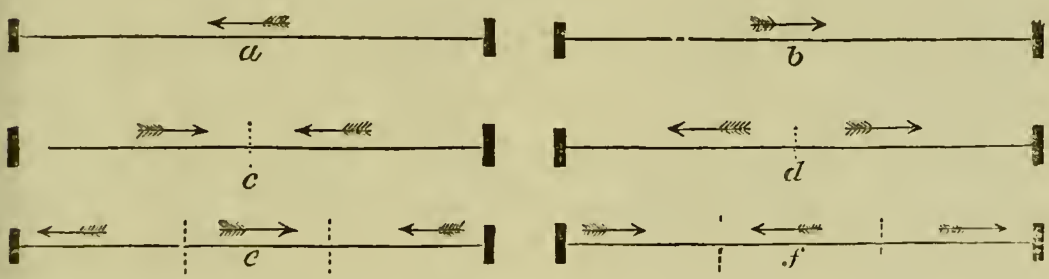
Causing the wire to vibrate again longitudinally through its entire length, my assistant shall at the same time turn the key at the end, thus changing the tension. You notice no variation of the note. The longitudinal vibrations of the wire, unlike the transverse ones, are independent of the tension. Beside the iron wire is stretched a second one of brass, of the same length and thickness. I rub them both. Their tones are not the same; that of the iron wire is considerably the higher of the two. Why? Simply because the velocity of the sound-pulse is greater in iron than in brass. The pulses in this case pass to and fro from end to end of the wire. At one moment the wire pushes the tray at its end; at the next moment it pulls the tray, this pushing and pulling being due to the passage of the pulse to and fro along the whole wire. The time required for a pulse to run from one end to the other *and back* is that of a complete vibration. In that time the wire imparts one push and one pull to the wooden tray at its end; the wooden tray imparts one complete vibration to the air, and the air bends once in and once out the tympanic membrane. It is manifest that the rapidity of vibration, or, in other words, the pitch of the note, depends upon the velocity with which the sound-pulse is transmitted through the wire.

And now the solution of a beautiful problem falls of itself into our hands. By shortening the brass wire we cause it to emit a note of the same pitch as that emitted by the other. You hear both notes now sounding in unison, and the reason is that the sound-pulse travels through these 23 feet of iron wire, and through these 15 feet 6 inches of brass wire, in the same time. These lengths are in the ratio of 11 : 17, and these two numbers

express the relative velocities of sound in brass and iron. In fact, the former velocity is 11,000 feet, and the latter 17,000 feet a second. The same method is of course applicable to many other metals.

When a wire or string, vibrating longitudinally, emits its lowest note, there is no node whatever upon it; the pulse, as just stated, runs to and fro along the whole length. But, like a string vibrating transversely, it can also subdivide itself into ventral segments separated by nodes. By damping the centre of the wire we make that point a node. The pulses here run from both ends, meet in the centre, recoil from each other, and return to the ends, where they are reflected as before. The note pro-

Fig. 82.



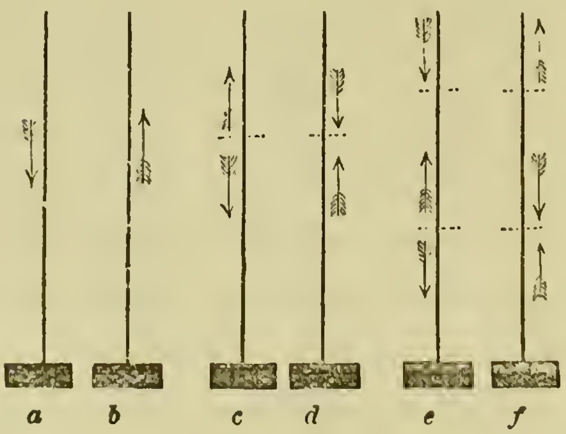
duced is the octave of the fundamental note. The next higher note corresponds to the division of the wire into three vibrating segments, separated from each other by two nodes. The first of these three modes of vibration is shown in fig. 82, *a* and *b*; the second at *c* and *d*; the third at *e* and *f*; the nodes being marked by dotted transverse lines, and the arrows in each case pointing out the direction in which the pulse moves. The rates of vibration follow the order of the numbers, 1, 2, 3, 4, 5, &c., just as in the case of a wire vibrating transversely.

A *rod* or *bar* of wood or metal, with its two ends fixed, and vibrating longitudinally, divides itself in the same manner as the wire. The succession of tones is also the same in both cases.

§ 3. *Longitudinal Vibrations of Rods fixed at one end: Musical Instruments formed on this principle.*

Rods and bars *with one end fixed* are also capable of vibrating longitudinally. A smooth wooden or metal rod, for example, with one of its ends fixed in a vice, yields a musical note, when the resined fingers are passed along it. When such a rod yields its lowest note, it simply elongates and shortens in quick alternation; there is, then, no node upon the rod. The pitch of the note is inversely proportional to the length of the rod. This follows necessarily from the fact that the time of a complete vibration is the time required for the sonorous pulse to run twice, to and fro, over the rod. The first overtone of a rod, fixed at one end, corresponds to its division by a node at a point one-third of its length from its free end. Its second overtone corresponds to a division by two nodes, the highest of which is at a point one-fifth of the length of the rod from its free end, the remainder of the rod being divided into two equal parts by the second node. In fig. 83, at *a* and *b*, *c* and *d*, *e* and *f*, are shown the conditions of the rod answering to its first three modes of vibration: the nodes, as before, are marked by dotted lines, the arrows in the respective cases marking the direction of the pulses.

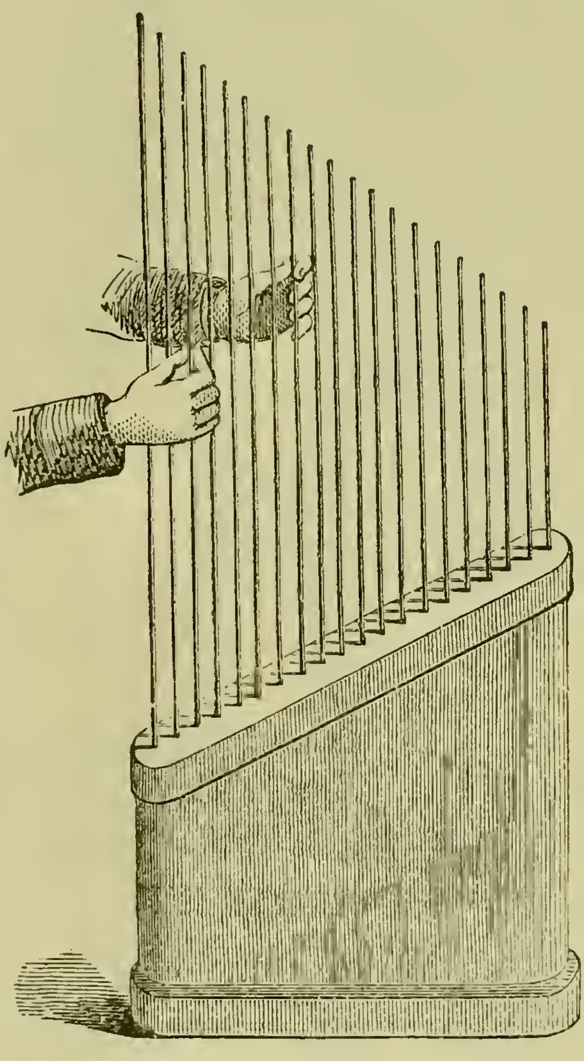
Fig. 83.



The order of the tones of a rod fixed at one end and vibrating longitudinally is that of the odd numbers 1, 3, 5, 7, &c. It is easy to see that this must be the case. For the time of vibration of *c* or *d* is that of the segment above the dotted line: and the length of this segment

being only one-third that of the whole rod, its vibrations must be three times as rapid. The time of vibration in *e* or *f* is also that of its highest segment, and as this segment is one-fifth of the length of the whole rod, its vibrations must be five times as rapid. Thus the order of the tones must be that of the odd numbers.

Fig. 84.



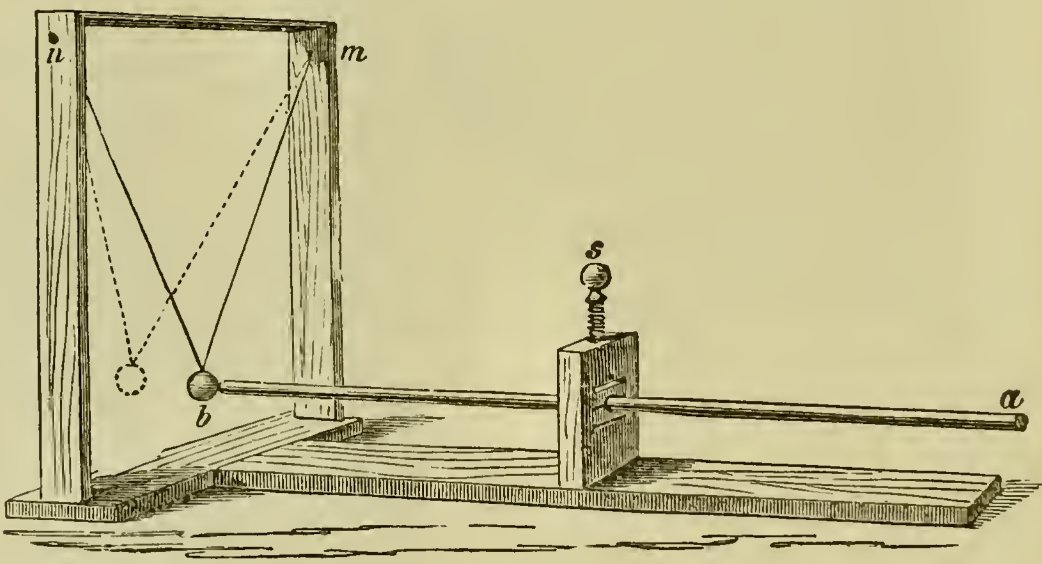
Before you, fig. 84, is a musical instrument, the sounds of which are due to the longitudinal vibrations of a number of deal rods of different lengths. Passing the resined fingers over the rods in succession, a series of notes of varying pitch is obtained. An expert performer might render the tones of this instrument very pleasant to you.

§ 4. *Vibrations of Rods free at both ends.*

Rods *with both ends free* are also capable of vibrating longitudinally, and producing musical tones. The investigation of this subject will lead us to exceedingly important results. Clasping a long glass tube exactly at its centre, and passing a wetted cloth over one of its halves, a clear musical sound is the result. A solid glass rod of the same length would yield the same note. In this case the centre of the tube is a node, and the two halves elongate and shorten in quick alternation. M. König of Paris has pro-

vided us with an instrument which will illustrate this action. A rod of brass, *a b*, fig. 85, is held at its centre by the clamp, *s*, while an ivory ball, suspended by two strings from the points, *m* and *n*, of a wooden frame, is caused to rest against the end, *b*, of the brass rod. Drawing gently a bit of resined leather over the rod near *a*, it is thrown into longitudinal vibration. The centre, *s*, is a node; but the uneasiness of the ivory ball shows you that the end, *b*, is in a state of tremor. I apply the rubber

Fig. 85.



still more briskly. The ball, *b*, rattles, and now the vibration is so intense, that the ball is rejected with violence whenever it comes into contact with the end of the rod.

§ 5. *Fracture of Glass Tube by Sonorous Vibrations.*

When the wetted cloth is passed over the surface of a glass tube the film of liquid left behind by the cloth is seen forming narrow tremulous rings all along the rod. Now, this shivering of the liquid is due to the shivering of the glass underneath it, and it is possible so to augment the intensity of the vibration that the glass shall actually go to pieces. Savart was the first to show this. Twice

in this place I have repeated this experiment, sacrificing in each case a fine glass tube 6 feet long and 2 inches in diameter. Seizing the tube at its centre C, fig. 86, I swept my hand vigorously to and fro along C D, until finally the half most distant from my hand was shivered into annular fragments. On examining these it was found that, narrow as they were, many of them were marked by circular cracks indicating a still more minute subdivision.

FIG. 86.

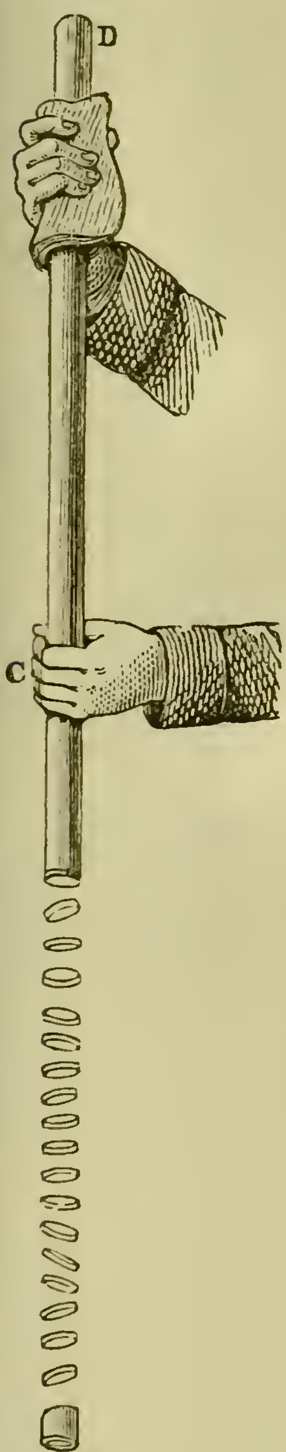
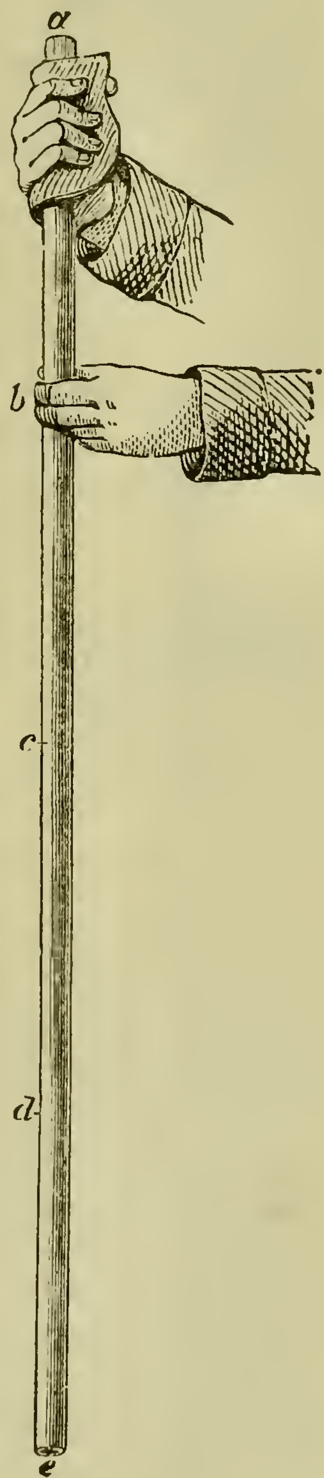


FIG. 87.

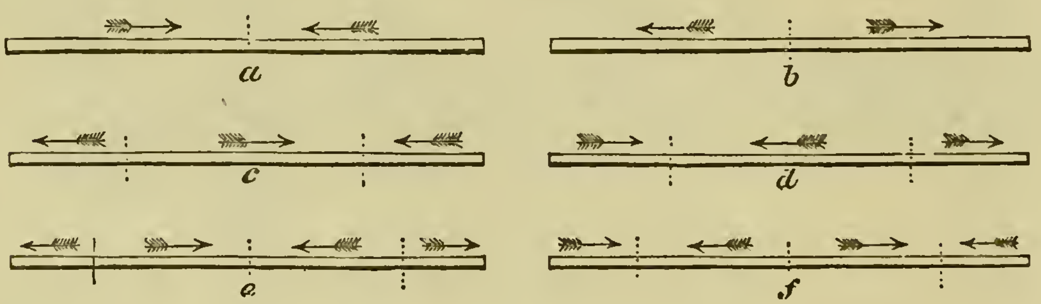


In this case also the rapidity of vibration is inversely proportional to the length of the rod. A rod of half the length vibrates longitudinally with double the rapidity, a rod of one-third the length with treble the rapidity, and so on. The time of a complete vibration being that required by the pulse to travel to and fro over the rod, and that time being directly proportional to the length of the rod, the rapidity of vibration must, of necessity, be in the inverse proportion.

This division of a rod by a single node at its centre corresponds to the deepest tone produced by its longitudinal vibration. But, as in all other cases hitherto examined, such rods can subdivide themselves further. Holding the long glass rod *a e*, fig. 87, at a point *b*, midway between its centre and one of its ends, and rubbing its short section, *a b*, with a wet cloth, the point *b* becomes a node, a second node, *d*, being formed at the same distance from the opposite end of the rod. Thus we have the rod divided into three vibrating parts, consisting of one whole ventral segment, *b d*, and two half ones, *a b* and *d e*. The sound corresponding to this division of the rod is the octave of its fundamental note.

You have now a means of checking me. For, if the second mode of division just described produces the octave

FIG. 88.



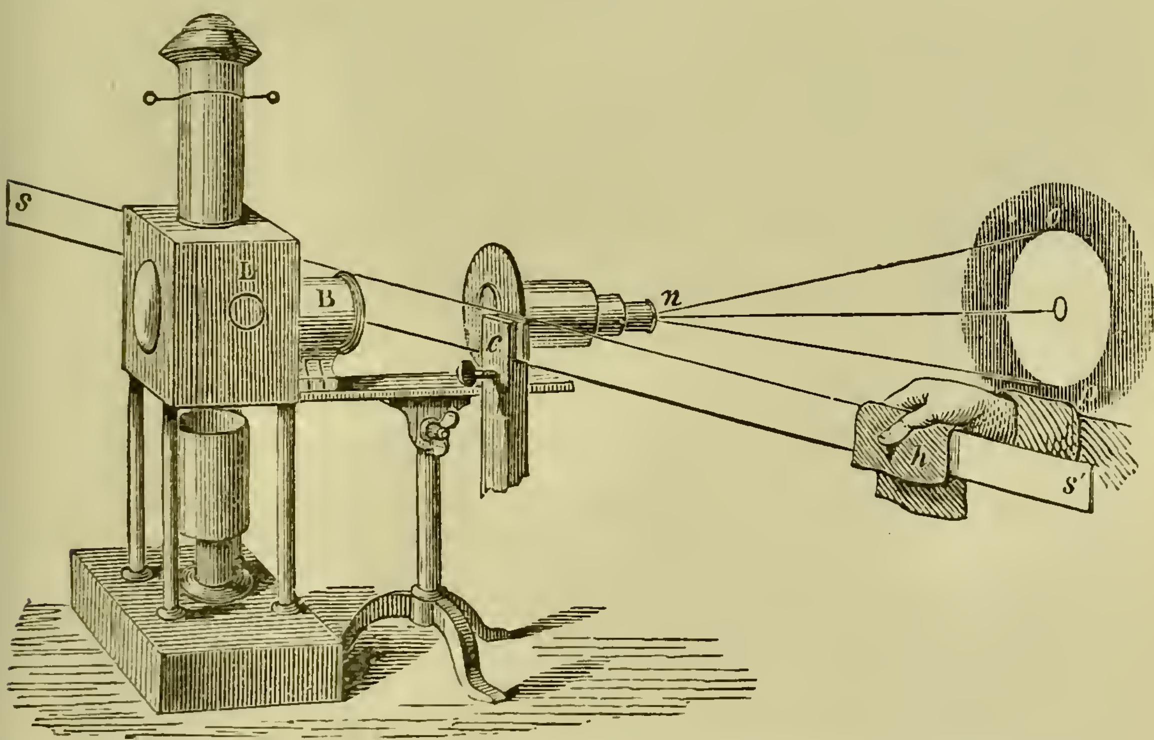
of the fundamental note, and if a rod of half the length produces the same octave, then the whole rod held at a point one-fourth of its length from one of its ends ought to emit the same note as the half rod held in the middle. When both notes are sounded together, they are heard to be identical in pitch.

Fig. 88, *a* and *b*, *c* and *d*, *e* and *f*, shows the three first divisions of a rod free at both ends and vibrating longitudinally. The nodes, as before, are marked by transverse dots, the direction of the pulses being shown by arrows. The order of the tones is that of the numbers 1, 2, 3, 4, &c.

§ 6. *Action of Sonorous Vibrations on Polarised Light.*

When a tube or rod vibrating longitudinally yields its fundamental tone, its two ends are in a state of free vibration, the glass there suffering neither strain nor pressure. The case at the centre is exactly the reverse; here there is no vibration, but a quick alternation of tension and compression. When the sonorous pulses meet at the centre they squeeze the glass; when they recoil they strain it.

Fig. 89.



Thus while at the ends we have the maximum of vibration, but no change of density, at the centre we have the maximum changes of density, but no vibration.

We have now cleared the way for the introduction of a very beautiful experiment made many years ago by Biot, but never, to my knowledge, repeated on the scale here presented to you. The beam from our electric lamp, L, fig. 89, being sent through a Nicol's prism, B, a beam of polarised light is obtained. This beam impinges on a second prism, *n*, but, though both prisms are perfectly transparent, the light which has passed through the first

cannot get through the second. By introducing a piece of glass between the two prisms, and subjecting the glass to either strain or pressure, in the proper direction, the light is enabled to pass through the entire system.

I now introduce horizontally between the prisms B and n a rectangle, s s', of plate glass, 6 feet long, 2 inches wide, and $\frac{1}{4}$ of an inch thick, the planes of vibration of the prisms being oblique to the horizon. The beam from L passes through the glass at a point near its centre, which is held in a vice, c, so that when a wet cloth is passed over one of the halves, c s, of the strip, the centre will be a node. During its longitudinal vibration the glass near the centre is, as already explained, alternately strained and compressed; and this successive strain and pressure so changes the condition of the light as to enable it to pass through the second prism. The screen is now dark; but on passing the wetted cloth briskly over the glass a brilliant disc of light, three feet in diameter, flashes out upon the screen. The vibration quickly subsides, and the luminous disc as quickly disappears, to be, however, revived at will by the passage of the wetted cloth along the glass.

The light of this disc appears to be continuous, but it is really intermittent, for it is only when the glass is under strain or pressure that the light can get through. In passing from strain to pressure, and from pressure to strain, the glass is for a moment in its natural state, which, if it continued, would leave the screen dark. But the impressions of brightness, due to the strains and pressures, remain far longer upon the retina than is necessary to abolish the intervals of darkness: hence the screen appears illuminated by a continuous light. When the glass rectangle is shifted so as to cause the beam of polarised light to pass through it close to its end, s, the longitudinal vibrations of the glass have no effect whatever upon the polarised beam.

Thus, by means of this subtle investigator, we demonstrate that while the centre of the glass, where the vibration is *nil*, is subjected to quick alternations of strain and pressure, the ends of the rectangle, where the vibration is a maximum, suffer neither.[1]

§ 7. *Vibrations of Rods of Wood: determination of Relative Velocities in different woods.*

Rods of wood and metal also yield musical tones when they vibrate longitudinally. Here, however, the rubber employed is not a wet cloth, but a piece of leather covered with powdered resin. The resined fingers also elicit the music of the rods. The modes of vibration here are those already indicated, the pitch, however, varying with the velocity of the sonorous pulse in the respective substances. When two rods of the same length, the one of deal, the other of Spanish mahogany, are sounded together, the pitch of the one is much lower than that of the other. Why? simply because the sonorous pulses pass more slowly through the mahogany than through the deal. Can we find the relative velocity of sound through both? With the greatest ease. We have only to carefully shorten the mahogany rod till it yields the same note as the deal one. The notes, rendered approximate by the first trials, are now identical. Through this rod of mahogany 4 feet long, and through this rod of deal 6 feet long, the sound-pulse passes in the same time, and these numbers express the relative velocities of sound through the two substances.

Modes of investigation, which could only be hinted at in our earlier lectures, are now falling naturally into our hands. When in the first lecture the velocity of sound in

[1] This experiment succeeds almost equally well with a glass tube. The coloured rings of a concave plate of selenite may be reversed by the sounding strip of glass.

air was spoken of, many possible methods of determining this velocity must have occurred to your minds, because here we have miles of space to operate upon. Its velocity through wood or metal, where such distances are out of the question, is determined in the simple manner just indicated. From the notes which they emit when suitably prepared, we may infer with certainty the *relative* velocities of sound through different solid substances; and determining the ratio of the velocity in any one of them to its velocity in air, we are able to draw up a table of absolute velocities. But how is air to be introduced into the series? We shall soon be able to answer this question, approaching it, however, through a number of phenomena with which, at first sight, it appears to have no connection.

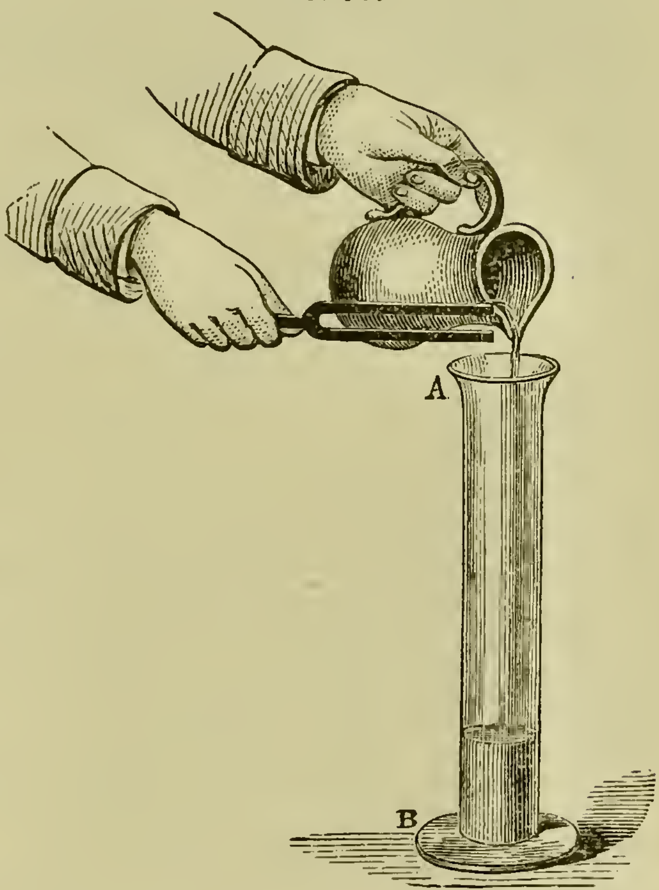
RESONANCE.

§ 8. *Experiments with Resonant Jars. Analysis and Explanation.*

The series of tuning-forks now before you have had their rates of vibration determined by the syren. One, you will remember, vibrates 256 times in a second, the length of its sonorous wave being 4 feet 4 inches. It is detached from its case, so that when struck against a pad you hardly hear it. When held over this glass jar, A B, fig. 90, 18 inches deep, you still fail to hear the sound of the fork. Preserving the fork in its position, I pour water with the least possible noise into the jar. The column of air underneath the fork shortens, the sound augments in intensity, and when the water has reached a certain level it bursts forth with extraordinary power. A greater quantity of water causes the sound to sink, and become finally inaudible, as at first. By pouring the water carefully out a point is reached where the reinforcement of the sound

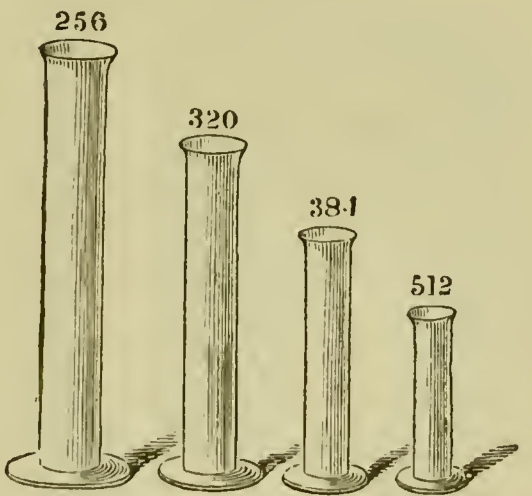
again occurs. Experimenting thus, we learn that there is one particular length of the column of air which, when the fork is placed above it, produces a maximum augmentation of the sound. This reinforcement of the sound is named *resonance*.

Fig. 90.



Operating in the same way with all the forks in succession, a column of air is found for each which yields a maximum resonance. These columns become shorter as the rapidity of vibration increases. In fig. 91, the series of jars is represented, the number of vibrations to which each resounds being placed above it.

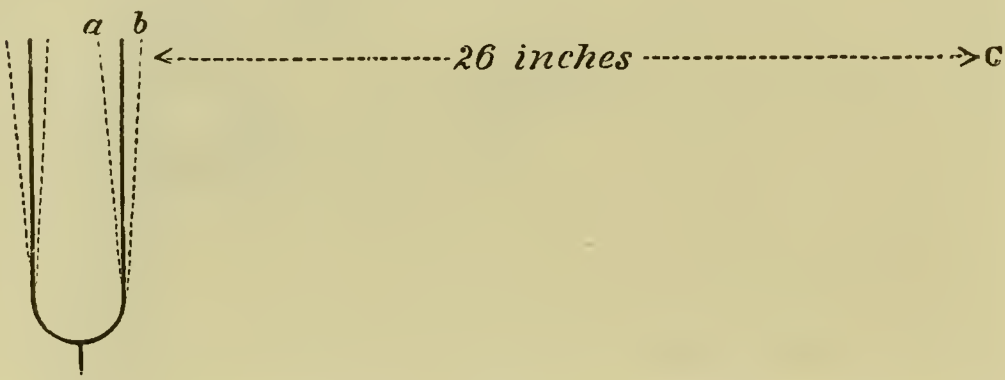
Fig. 91.



What is the physical meaning of this very wonderful effect? To solve this question we must revive our knowledge of the relation of the motion of the fork itself to the motion of the sonorous wave produced by the fork. Supposing a prong of this fork, which executes 256 vibrations in a second, to vibrate between the points *a* and *b*, fig. 92. In its motion from *a* to *b* the fork generates half a sonorous wave, and as the length of the whole wave emitted by

this fork is 4 feet 4 inches, at the moment the prong reaches *b* the foremost point of the sonorous wave will be at C, 2 feet 2 inches distant from the fork. The motion of the wave, then, is vastly greater than that of the fork.

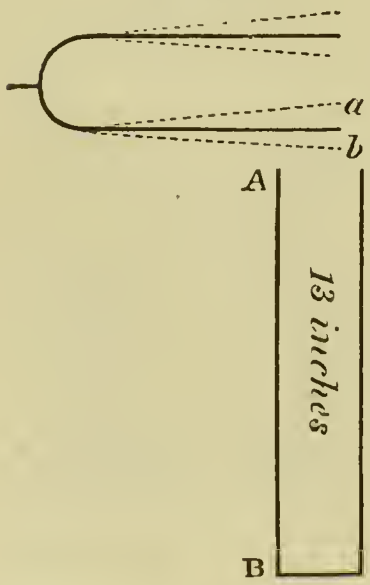
FIG. 92.



In fact, the distance *a b* is, in this case, not more than one-twentieth of an inch, while the wave has passed over a distance of 26 inches. With forks of lower pitch the difference would be still greater.

Our next question is, what is the length of the column of air which resounds to this fork? By measurement with a two-foot rule it is found to be 13 inches. But the length of the wave emitted by the fork is 52 inches; hence *the length of the column of air which resounds to the fork is equal to one-fourth of the length of the sound-wave produced by the fork.* This rule is general, and might be illustrated by any other of the forks instead of this one.

FIG. 93.



Let the prong, vibrating between the limits *a* and *b*, be placed over its resonant jar, AB, fig. 93. In the time required by the prong to move from *a* to *b*, the condensation it produces runs down to the bottom of the jar, is there reflected, and, as the distance to the bottom and back is 26 inches, the reflected wave will reach the fork

at the moment when it is on the point of returning from *b* to *a*. The rarefaction of the wave is produced by the retreat of the prong from *b* to *a*. This rarefaction will also run to the bottom of the jar and back, overtaking the prong just as it reaches the limit, *a*, of its excursion. It is plain from this analysis that the vibrations of the fork are perfectly synchronous with the vibrations of the aerial column A B; and in virtue of this synchronism the motion accumulates in the jar, spreads abroad in the room, and produces this vast augmentation of the sound.

When we substitute for the air in one of these jars a gas of different elasticity, we find the length of the resounding column to be different. The velocity of sound through coal-gas is to its velocity in air about as 8 : 5. Hence, to synchronise with our fork, a jar filled with coal-gas must be deeper than one filled with air. I turn this jar, 18 inches long, upside down, and hold close to its open mouth our agitated tuning-fork. It is scarcely audible. The jar, with air in it, is 5 inches too deep for this fork. Let coal-gas now enter the jar. As it ascends the note at a certain point swells out, proving that for the more elastic gas a depth of 18 inches is not too great. In fact, it is not great enough; for if too much gas be allowed to enter the jar the resonance is weakened. By suddenly turning the jar upright, still holding the fork close to its mouth, the gas escapes, and at the point of proper admixture of gas and air the note swells out again.[1] Guided by these principles Professor George Forbes has devised a very elegant little instrument for the detection of fire-damp. From the length of the column which resounds to a tuning-fork of definite pitch, he infers the percentage of marsh gas in the air of the mine.

[1] This experiment is more easily executed with hydrogen than with coal-gas.

§ 9. *Reinforcement of Bell by Resonance.*

This fine sonorous bell, fig. 94, is thrown into intense vibration by the passage of a resined bow across its edge. You hear its sound, but it is not very forcible. When, however, the open mouth of a large tube, closed at one end, is brought close to one of the vibrating segments of the bell, the tone swells into a musical roar. As the tube is alternately withdrawn and advanced, the sound sinks and swells in an extraordinary manner.

A second tube, open at both ends, is capable of being lengthened and shortened by a telescopic slider. When

Fig. 94.

brought near the vibrating bell, the resonance is feeble. On lengthening the tube by drawing out the slider, at a certain point the tone swells out as before. If the tube be made longer, the resonance is again enfeebled. Note the fact, which shall be explained presently, that the open tube which gives the maximum resonance is exactly twice the length of the closed one. For these fine experiments we are indebted to Savart.

§ 10. *Expenditure of Motion in Resonance.*

With the india-rubber tube employed in our third chapter it was found necessary to time the impulses properly, so as to produce the various ventral segments. I could then feel that the muscular work performed, when the impulses were properly timed, was greater than when they were irregular. The same truth may be illustrated by a claret glass half filled with water. Endeavour to move your hand to and fro in accordance with the oscillating period of the water: when you have thoroughly established synchronism, the work thrown upon the hand apparently augments the weight of the water. So likewise with our tuning-fork; when its impulses are timed to the vibrations of the column of air contained in this jar, its work is greater than when they are not so timed. As a consequence of this the tuning-fork comes sooner to rest when it is placed over the jar than when it is permitted to vibrate either in free air, or over a jar of a depth unsuited to its periods of vibration.[1]

Reflecting on what we have now learned, you would have little difficulty in solving the following beautiful problem:—You are provided with a tuning-fork and a syren, and are required by means of these two instruments to determine the velocity of sound in air. To solve this problem, you lack, if anything, the mere power of manipulation which practice imparts. You would first determine by means of the syren the number of vibrations executed by the tuning-fork in a second; you would then determine the length of the column of air which resounds to the fork. This length multiplied by 4 would give you,

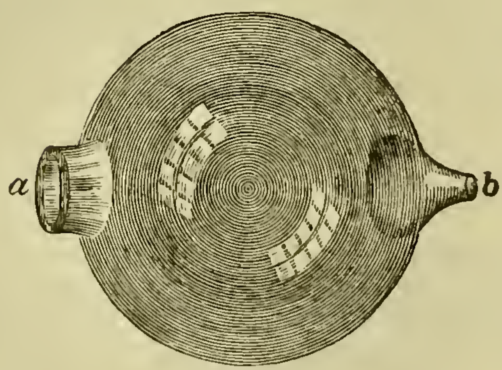
[1] Only an extremely small fraction of the fork's motion is, however, converted into sound. The remainder is expended in overcoming the internal friction of its own particles. In other words, nearly the whole of the motion is converted into heat.

approximately, the wave-length of the fork, and the wave-length, multiplied by the number of vibrations in a second, would give you the velocity in a second. Without quitting your private room, therefore, you could solve this important problem. We will go on, if you please, in this fashion, making our footing sure as we advance.

§ 11. *Resonators of Helmholtz.*

Helmholtz has availed himself of the principle of resonance in analysing composite sounds. He employs little hollow spheres called *resonators*, one of which is shown in fig. 94*a*. The small projection *b*, which has an orifice, is placed in the ear, while the sound-waves enter the hollow sphere through the wide aperture at *a*. Reinforced by the resonance of such a cavity, and rendered thereby more powerful than its companions, a particular note of a composite clang may be in a measure isolated and studied alone.

Fig. 94*a*.



ORGAN-PIPES.

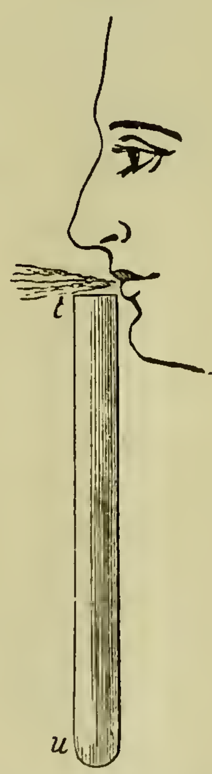
§ 12. *Principles of Resonance applied to Organ-Pipes.*

Thus disciplined we are prepared to consider the subject of organ-pipes, which is one of great importance. Before me on the table are two resonant jars, and in my right hand and my left are held two tuning-forks. I agitate both, and hold them over this jar. One of them only is heard. Held over the other jar, the other fork alone is heard. Each jar selects that fork whose periods of

vibration synchronise with its own. And instead of two forks, suppose several of them to be held over the same jar; from the confused assemblage of pulses thus generated, the jar would select and reinforce that one which corresponds to its own period of vibration.

When I blow across the open mouth of the jar; or, better still, for the jar is too wide for this experiment, when I blow across the open end of a glass tube, *t u*, fig. 95, of the same length as the jar, a fluttering of the air is thereby produced, an assemblage of pulses at the open mouth of the tube being generated. And what is the consequence? The tube selects that pulse of the flutter which is in synchronism with itself, and raises it to a musical sound. The sound, you perceive, is precisely that obtained when the proper tuning-fork is placed over the tube. The column of air within the tube has, in this case, virtually created its own tuning-fork; for by the reaction of its pulses upon the sheet of air issuing from the lips it has compelled that sheet to vibrate in synchronism with itself, and made it thus act the part of the tuning-fork.

Fig. 95.



Selecting for each of the other tuning-forks a resonant tube, in every case, on blowing across the open end of the tube, a tone is produced identical in pitch with that obtained through resonance.

When different tubes are compared, the rate of vibration is found to be inversely proportional to the length of the tube. These three tubes are 24, 12, and 6 inches long, respectively. I blow gently across the 24-inch tube, and bring out its fundamental note; similarly treated, the 12-inch tube yields the octave of the note of the 24-inch. high
In like manner the 6-inch tube yields the octave of the

12-inch. It is plain that this must be the case; for the rate of vibration depending on the distance which the pulse has to travel to complete a vibration, if in one case this distance be twice what it is in another, the rate of vibration must be twice as slow. In general terms the rate of vibration is inversely proportional to the length of the tube through which the pulse passes.

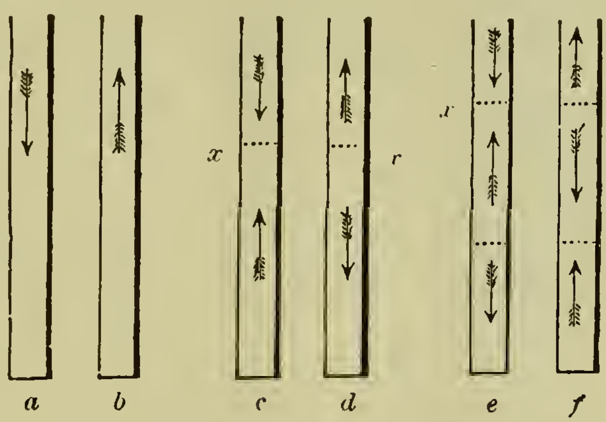
§ 13. *Vibrations of Stopped Pipes: modes of division: Overtones.*

But that the current of air should be thus able to accommodate itself to the requirements of the tube, it must enjoy a certain amount of *flexibility*. A little reflection will show you that the power of the reflected pulse over the current must depend to some extent on the force of the current. A stronger current, like a more powerfully stretched string, requires a greater force to deflect it, and when deflected vibrates more quickly. Accordingly, to obtain the fundamental note of this 24-inch tube, we must blow very gently across its open end; a rich, full, and forcible musical tone is then produced. With a little stronger blast the sound approaches a mere rustle; blowing stronger still, a tone is obtained of much higher pitch than the fundamental one. This is the first overtone of the tube, to produce which the column of air has divided itself into two vibrating parts, with a node between them. With a still stronger blast a still higher note is obtained. The tube is now divided into three vibrating parts, separated from each other by two nodes. Once more I blow with sudden strength; a higher note than any before obtained is the consequence.

In fig. 96 are represented the divisions of the column of air corresponding to the first three notes of a tube stopped at one end. At *a* and *b*, which correspond to the

fundamental note, the column is undivided; the bottom of the tube is the only node, and the pulse simply moves up and down from top to bottom, as denoted by the arrows. In *c* and *d*, which correspond to the first overtone of the tube, we have one nodal surface shown by dots at x, against which the pulses abut, and from which they are reflected as from a fixed surface. This nodal surface is situated at one-third of the length of the tube from its open end. In *e* and *f*, which correspond to the second overtone, we have two nodal surfaces, the upper one, x', of which is at one-fifth of the length of the tube from its open end, the remaining four-fifths being divided into two equal parts by the second nodal surface. The arrows, as before, mark the directions of the pulses.

Fig. 96.



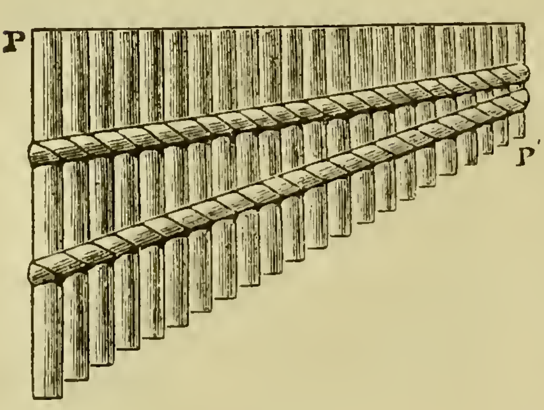
We have now to inquire into the relation of these successive notes to each other. The space from node to node has been called all through 'a ventral segment'; hence the space between the middle of a ventral segment and a node is a semi-ventral segment. You will readily bear in mind the law, that *the number of vibrations is directly proportional to the number of semi-ventral segments* into which the column of air within the tube is divided. Thus when the fundamental note is sounded, we have but a single semi-ventral segment, as at *a* and *b*. The bottom here is a node, and the open end of the tube, where the air is agitated, is the middle of a ventral segment. The mode of division represented in *c* and *d* yields three semi-ventral segments; in *e* and *f* we have five. The vibrations, therefore, corresponding to this series of notes,

augment in the proportion of the series of odd numbers, 1 : 3 : 5. And could we obtain still higher notes, their relative rates of vibration would continue to be represented by the odd numbers, 7, 9, 11, 13, &c. &c.

It is evident that this *must* be the law of succession. For the time of vibration in *c* or *d* is that of a stopped tube of the length *x y*; but this length is one-third of the length of the whole tube, consequently its vibrations must be three times as rapid. The time of vibration in *e* or *f* is that of a stopped tube of the length *x′ y′*, and inasmuch as this length is one-fifth that of the whole tube, its vibrations must be five times as rapid. We thus obtain the succession 1, 3, 5, and if we pushed matters further we should obtain the continuation of the series of odd numbers.

It is now once more in your power to subject my statements to an experimental test. Here are two tubes, one of which is three times the length of the other. I sound the fundamental note of the longest tube, and then the next note above the fundamental. The vibrations of these two notes are stated to be in the ratio of 1 : 3. This latter note, therefore, ought to be of precisely the same pitch as the fundamental note of the shorter of the two tubes. When both tubes are sounded their notes are identical.

Fig. 97.



It is only necessary to place a series of such tubes of different lengths thus together to form that ancient instrument, Pan's pipes, P P′, fig. 97, with which we are so well acquainted.

The successive divisions, and the relation of the overtones of a rod fixed at one end (described in p. 163), are

plainly identical with those of a column of air in a tube stopped at one end, which we have been here considering.

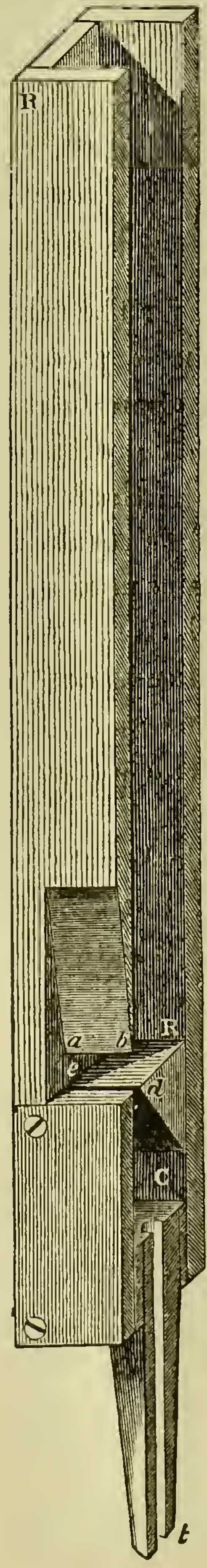
§ 14. *Vibrations of Open Pipes: modes of division: Overtones.*

From tubes closed at one end, and which, for the sake of brevity, may be called stopped tubes, we now pass to tubes open at both ends, which we shall call open tubes. Comparing, in the first instance, a stopped tube with an open one of the same length, we find the note of the latter an octave higher than that of the former. This result is general. To make an open tube yield the same note as a closed one, it must be twice the length of the latter. And since the length of a closed tube sounding its fundamental note is one-fourth of the length of its sonorous wave, the length of an open tube is one-half that of the sonorous wave that it produces.

It is not easy to obtain a sustained musical note by blowing across the end of an open glass tube; but a mere puff of breath across the end reveals the pitch to the disciplined ear. In each case it is that of a closed tube half the length of the open one.

There are various ways of agitating the air at the ends of pipes and tubes so as to throw the air-columns within them into vibration. In organ-pipes this is done by blowing a thin sheet of air against a sharp edge. You will have no difficulty in understanding the construction of an organ-pipe, open at the top, from this model, fig. 98, one side of which has been removed so that you may see its inner parts. Through the tube *t* the air passes from the wind-chest into the chamber, C, which is closed above, save a narrow slit, *e d*, through which the compressed air of the chamber issues. This thin air current breaks against

Fig. 98.



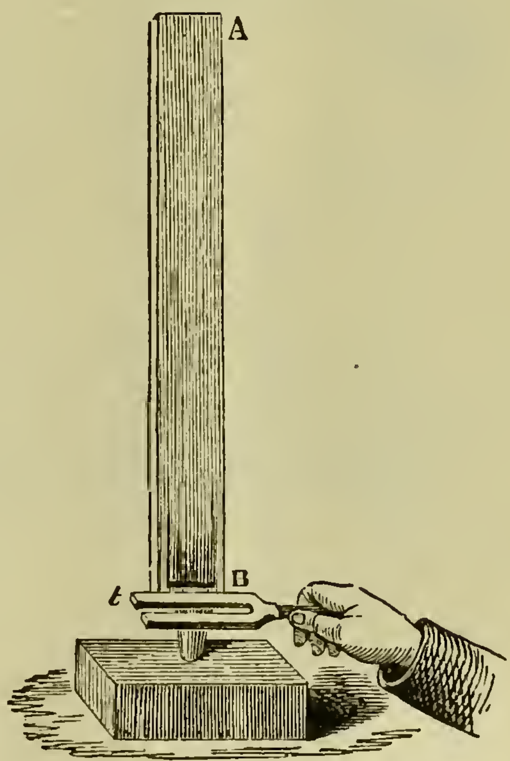
the sharp edge, *a b*, and there produces a fluttering noise, and the proper pulse of this flutter is converted by the resonance of the pipe above into a musical sound. The open space between the edge, *a b*, and the slit below it is called the *embouchure.* Fig. 99 represents a stopped pipe of the same length as that shown in fig. 98, and hence producing a note an octave lower.

Instead of a fluttering sheet of air, a tuning-fork whose rate of vibration synchronises with that of the organ pipe may be placed at the embouchure, as at A B, fig. 100. The pipe will resound. Here, for example, are four open pipes of different lengths, and four tuning-forks of different rates of vibration. Striking the most slowly vibrating fork, and bringing it near the embouchure of the longest pipe, the pipe *speaks* powerfully. When blown into, the same pipe yields a tone identical with that of the tuning-fork. Going through all the pipes in succession, we find, in each case, that the note obtained by blowing into the pipe is exactly that produced

Fig. 99.

when the proper tuning-fork is placed at the embouchure. Conceive now the four forks placed together near the same embouchure; we should have pulses of four different periods there excited; but out of the four the pipe would select only one. And if four hundred vibrating forks could be placed there instead of four, the pipe would still make the proper selection. This it also does when for the pulses of tuning-forks we substitute the assemblage of pulses created by the current of air when it strikes against the sharp upper edge of the embouchure.

Fig. 100.

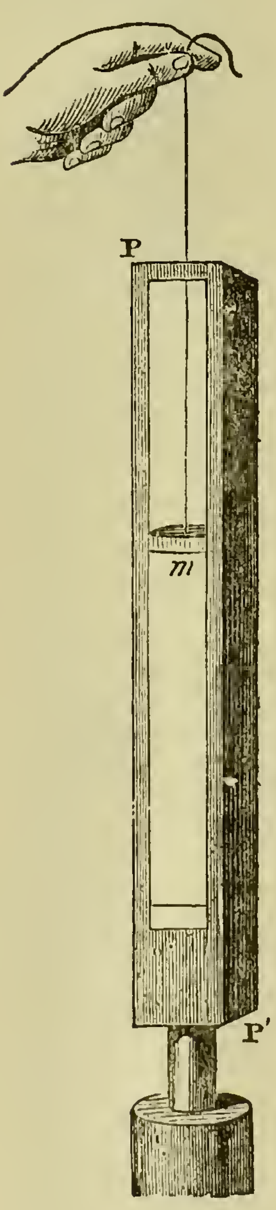


The heavy vibrating mass of the tuning-fork is practically uninfluenced by the motion of the air within the pipe. But this is not the case when air itself is the vibrating body. Here, as before explained, the pipe creates, as it were, its own tuning-fork, by compelling the fluttering stream at its embouchure to vibrate in periods answering to its own.

The condition of the air within an open organ-pipe when its fundamental note is sounded is that of a rod free at both ends, held at its centre, and caused to vibrate longitudinally. The two ends are places of vibration, the centre is a node. Is there any way of *feeling* the vibrating air-column so as to determine its nodes and its places of vibration? The late excellent William Hopkins has taught us the following mode of solving this problem. Over a little hoop is stretched a thin membrane, forming a little tambourine. The front of this organ-pipe, P P′,

fig. 101, is of glass, through which you can see the position of any body within it. By means of a string, the little tambourine, *m*, can be raised or lowered at pleasure through the entire length of the pipe. When held above the upper end of the pipe you hear the loud buzzing of the membrane. When lowered into the pipe it continues to buzz for a time; the sound becoming gradually feebler, and finally ceasing totally. It is now in the middle of the pipe, where it cannot vibrate, because the air around it is at rest. On lowering it still further, the buzzing sound instantly recommences, and continues down to the bottom of the pipe. Thus, as the membrane is raised and lowered in quick succession, during every descent and ascent, we have two periods of sound separated from each other by one of silence. If sand were strewn upon the membrane, it would dance above and below, but it would be quiescent at the centre. We thus prove experimentally, that when an organ-pipe sounds its fundamental note it divides itself into two semi-ventral segments separated by a node.

FIG. 101.

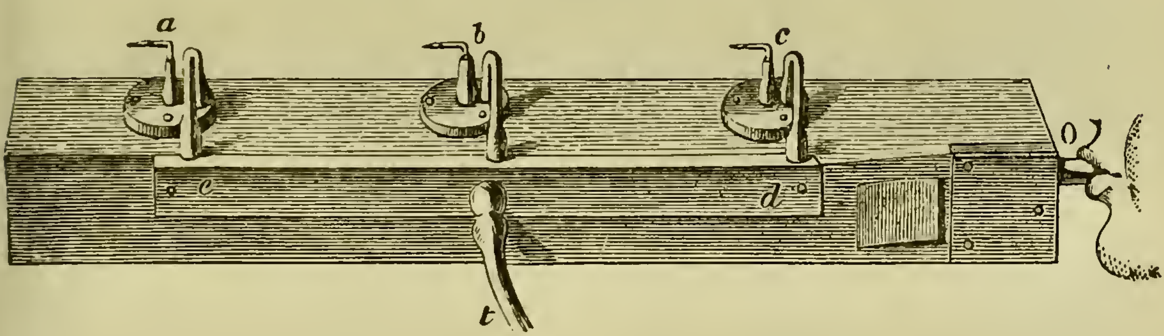


What is the condition of the air at this node? Again that of the middle of a rod, free at both ends, and yielding the fundamental note of its longitudinal vibration. The pulses reflected from both ends of the rod, or of the column of air, meet in the middle, and produce compression; they then retreat and produce rarefaction. Thus, while there is no vibration in the centre of an organ-pipe, the air there undergoes the greatest changes of density. At the two

ends of the pipe, on the other hand, the air-particles merely swing up and down without sensible compression or rarefaction.

If the sounding pipe were pierced at the centre, and the orifice stopped by a membrane, the air, when condensed, would press the membrane outwards, and, when rarefied, the external air would press the membrane inwards. The membrane would therefore vibrate in unison with the column of air. The organ-pipe, fig. 102, is so arranged that a small jet of gas, *b*, can be lighted opposite the centre of the pipe, and be there acted upon by the vibrations of a membrane. Two other gas jets, *a* and *c*, are placed nearly midway between the centre and the two ends of

Fig. 102.



the pipe. The three burners, *a*, *b*, *c*, are fed in the following manner:—Through the tube, *t*, the gas enters the hollow chamber, *e d*, from which issue three little bent tubes, shown in the figure, each communicating with a capsule closed underneath by the membrane. This is in direct contact with the air of the organ-pipe. From the three capsules issue the three little burners with their flames, *a*, *b*, *c*.

Blowing into the pipe so as to sound its fundamental note, the three flames are agitated, but the central one is most so. Lowering the flames so as to render them very small, and blowing again, the central flame, *b*, is extinguished, while the others remain lighted. The experiment may be performed half-a-dozen times in succession;

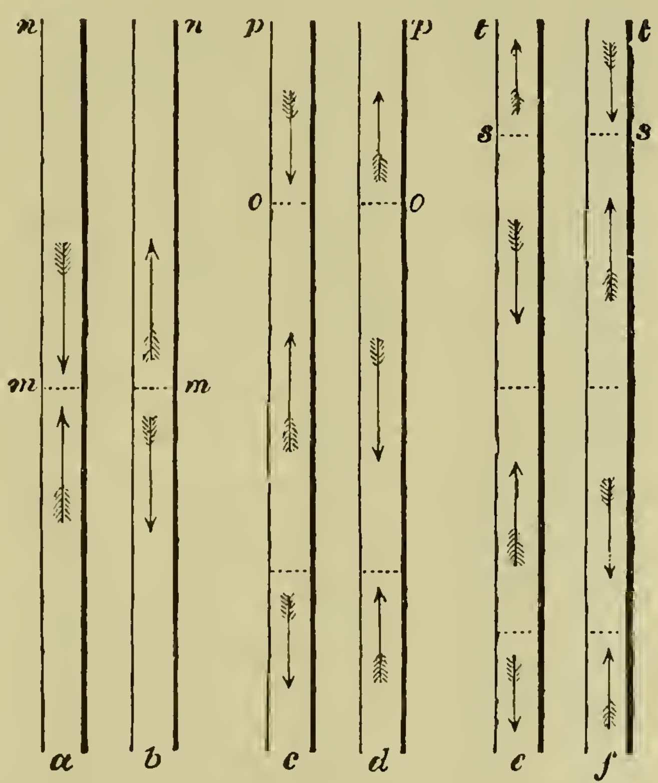
the sounding of the fundamental note always quenches the middle flame.

By blowing more sharply into the pipe, it is caused to yield its first overtone. The middle node no longer exists. At the centre of the pipe no variations of pressure now occur, while two nodes are formed midway between the centre and the two ends. But if this be the case, and if the flame opposite a node be always blown out, then, when the first overtone of this pipe is sounded, the two flames *a* and *c* ought to be extinguished, while the central flame remains lighted. This is the case. When the first harmonic is sounded the two nodal flames are infallibly extinguished, while the flame *b* in the middle of the ventral segment is not sensibly disturbed.

There is no theoretic limit to the subdivison of an organ-pipe, either stopped or open. In stopped pipes we begin with 1 semi-ventral segment, and pass on to 3, 5, 7, &c., semi-ventral segments; the number of vibrations of the successive notes augmenting in the same ratio. In open pipes we begin with 2 semi-ventral segments, and pass on to 4, 6, 8, 10, &c., the number of vibrations of the successive notes augmenting in the same ratio: that is to say, in the ratio 1 : 2 : 3 : 4 : 5, &c. When, therefore, we pass from the fundamental tone to the first overtone in an open pipe, we obtain the octave of the fundamental. When we make the same passage in a stopped pipe, we obtain a note a fifth above the octave. No intermediate modes of vibration are in either case possible. If the fundamental tone of a stopped pipe be produced by 100 vibrations a second, the first overtone will be produced by 300 vibrations, the second by 500, and so on. Such a pipe, for example, cannot execute 200 or 400 vibrations in a second. In like manner the open pipe, whose fundamental note is produced by 100 vibrations a second, cannot vibrate 150 times in a second, but passes, at a jump, to 200, 300, 400, and so on.

In open pipes, as in stopped ones, the number of vibrations executed in the unit of time is inversely proportional to the length of the pipe. This follows from the fact, already dwelt upon so often, that the time of a vibration is determined by the distance which the sonorous pulse has to travel to complete a vibration.

In fig. 103, *a* and *b* (at the bottom) represent the division of an open pipe corresponding to its fundamental tone; *c* and *d* represent the division corresponding to its first; *e* and *f* the division corresponding to its second overtone, the dots marking the nodes. The distance *m n* is one-half, *o p* is one-fourth, and *s t* is one-sixth of the whole length of the pipe. But the pitch of *a* is that of a stopped pipe equal in length to *m n*; the pitch of *c* is that of a stopped pipe of the length *o p*; while the pitch of *e* is that of a stopped pipe of the length *s t*. Hence, as these lengths are in the ratio of $\frac{1}{2} : \frac{1}{4} : \frac{1}{6}$, or as $1 : \frac{1}{2} : \frac{1}{3}$, the rates of vibration must be as the reciprocals of these, or as $1 : 2 : 3$. From the mere inspection, therefore, of the respective modes of vibration, we can draw the inference that the succession of tones of an open pipe must correspond to the series of natural numbers.

Fig. 103.

The pipe *a*, fig. 103, has been purposely drawn twice the length of *a*, fig. 96 (p. 179). It is perfectly manifest that to complete a vibration the pulse has to pass over the same distance in both pipes, and hence that the

pitch of the two pipes must be the same. The open pipe, *a n*, consists virtually of two stopped ones, with the central nodal surface at *m* as their common base. This shows the relation of a stopped pipe to an open one to be that which experiment establishes.

§ 15. *Velocity of Sound in Gases, Liquids, and Solids, determined by Musical Vibrations.*

We have already learned that the *relative* velocities of sound in different solid bodies may be determined from the notes which they emit when thrown into longitudinal vibration. It was remarked at the time, that to draw up a table of *absolute* velocities we only required the accurate comparison of the velocity in any one of those solids with the velocity in air. We are now in a condition to supply this comparison. For we have learned that the vibrations of the air in an organ-pipe open at both ends are executed precisely as those of a rod free at both ends. Any difference of rapidity, therefore, between the vibrations of a rod, and of an open organ-pipe of the same length, must be due solely to the different velocities with which the sonorous pulses are propagated through them. Take therefore an organ-pipe of a certain length, emitting a note of a certain pitch, and find the length of a rod of pine which yields the same note. This length would be ten times that of the organ-pipe, which would prove the velocity of sound in pine to be ten times its velocity in air. But the absolute velocity in air at a temperature of 0° C. is 1,090 feet a second; hence the absolute velocity in pine is 10,900 feet a second, which is that given in our first chapter (p. 40). To the celebrated Chladni we are indebted for this beautiful mode of determining the velocity of sound in solid bodies.

We had also in our first lecture a table of the velocities of sound in other gases than air. I am persuaded that

you could tell me, after due reflection, how this table was constructed. It would only be necessary to find a series of organ-pipes which, when filled with the different gases, yield the same note; the lengths of these pipes would give the relative velocities of sound through the gases. Thus we should find the length of a pipe filled with hydrogen to be four times that of a pipe filled with oxygen, yielding the same note, and this would prove the velocity of sound in the former to be four times its velocity in the latter.

But we had also a table of velocities through various liquids. How was it constructed? By forcing the liquids through properly constructed organ-pipes, and comparing their musical tones. Thus, in water it requires a pipe a little better than four feet long to produce the note of an air-pipe one foot long; and this proves the velocity of sound in water to be somewhat more than four times its velocity in air. My object here is to give you a clear notion of the way in which scientific knowledge enables us to cope with these apparently insurmountable problems. It is not necessary to go into the niceties of these measurements. You will, however, readily comprehend that all the experiments with gases might be made with the same organ-pipe, the velocity of sound in each respective gas being immediately deduced from the pitch of its note. With a pipe of constant length the pitch, or, in other words, the number of vibrations, would be directly proportional to the velocity. Thus, comparing oxygen with hydrogen, we should find the note of the latter to be the double octave of that of the former, whence we should infer the velocity of sound in hydrogen to be four times its velocity in oxygen. The same remark applies to experiments with liquids. Here also the same pipe may be employed throughout, the velocities being inferred from the notes produced by the respective liquids.

In fact, the length of an open pipe being, as already explained, one-half the length of its sonorous wave, it is only necessary to determine, by means of the syren, the number of vibrations executed by the pipe in a second, and to multiply this number by twice the length of the pipe, in order to obtain the velocity of sound in the gas or liquid within the pipe. Thus, an open pipe 26 inches long and filled with air executes 256 vibrations in a second. The length of its sonorous wave is twice 26 inches, or $4\frac{1}{3}$ feet: multiplying 256 by $4\frac{1}{3}$, we obtain 1,120 feet per second as the velocity of sound through air of the temperature of this room. Were the tube filled with carbonic acid gas, its vibrations would be slower: were it filled with hydrogen, its vibrations would be quicker; and applying the same principle, we should find the velocity of sound in both these gases.

So likewise the length of a solid rod free at both ends, and sounding its fundamental note, is half that of the sonorous wave in the substance of the solid. Hence we have only to determine the rate of vibration of such a rod, and multiply it by twice the length of the rod, to obtain the velocity of sound in the substance of the rod. You can hardly fail to be impressed by the power which physical science has given us over these problems; nor will you refuse your admiration to that famous old investigator, Chladni, who taught us how to master them experimentally.

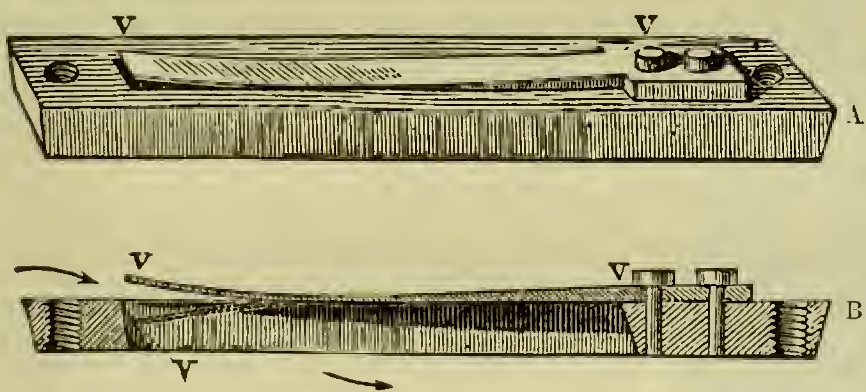
REEDS AND REED-PIPES.

The construction of the syren, and our experiments with that instrument, are, no doubt, fresh in your recollection. Its musical sounds are produced by the cutting up into puffs of a series of air-currents. The same purpose is effected by a vibrating reed, as employed in the

accordion, the concertina, and the harmonica. In these instruments it is not the vibrations of the reed itself which, imparted to the air, and transmitted through it to our organs of hearing, produce the music; the function of the reed is *constructive*, not *generative*; it moulds into a series of discontinuous puffs that which without it would be a continuous current of air.

Reeds, if associated with pipes, sometimes command, and are sometimes commanded by, the vibrations of the column of air. When they are stiff they rule the column; when they are flexible the column rules them. In the former case, to derive due advantage from the air-column, its length ought to be so regulated that either its

Fig. 104.



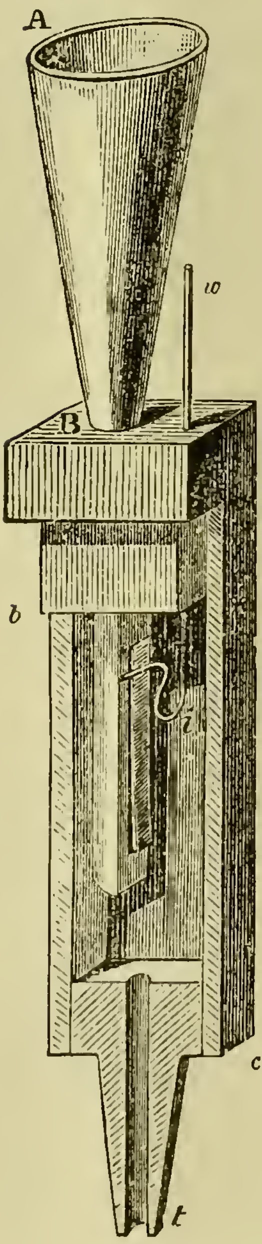
fundamental tone or one of its overtones shall correspond to the rate of vibration of the reed. A metal reed is shown in fig. 104, A and B, both in perspective and in section. It consists of a long flexible strip of metal, V V, placed in a rectangular orifice through which the current of air enters the pipe. The manner in which a reed and pipe are associated is shown in fig. 105. The front, *b c*, of the space containing the flexible tongue is of glass, so that you may see the tongue within it. A conical pipe, A B, surmounts the reed.[1] The wire *w i*, shown pressing against

[1] The illustrations of organ-pipes and reeds introduced here, and at p. 183, have been substantially copied from the excellent work of Helmholtz. Pipes opening with hinges so as to show their inner parts were shown in the lecture.

the reed, is employed to lengthen or shorten it, and thus to vary within certain limits its rate of vibration.

Reeds are of two kinds, the *free* reed and the *beating* reed. The former passes to and fro through the aperture without touching its sides. It is used in instruments of the harmonium and accordion class. The latter falls against the sides of the aperture, which it periodically closes and opens. The beating reed is used in the organ and in other instruments. In the organ the reed governs the pipe; in the clarinet and oboe, which have flexible wooden reeds, the pipe governs the reed. When a reed and its associated pipe synchronise perfectly, the sound is most pure and forcible; a certain latitude, however, is possible on both sides of perfect synchronism.

Fig. 105.



Perhaps the simplest illustration of the action of the reed commanded by its aerial column is furnished by a common wheaten straw. At about an inch from a knot, at r, I bury my pen-knife in this straw, $s\ r'$, fig. 106, to a depth of about one-fourth of the straw's diameter, and, turning the blade flat, pass it upwards towards the knot, thus raising a strip of the straw nearly an inch in length. This strip, $r\ r'$, is to be our reed, and the straw itself is to be our pipe. It is now eight inches long. When blown into, it emits this decidedly musical sound. When cut so as to make its length six inches, the pitch is higher; with a length of four inches, the pitch is higher still; and with a length of two inches, the sound is very shrill indeed. In these

experiments the reed was compelled to accommodate itself throughout to the requirements of the vibrating column of air.

The clarionet is a reed pipe. It has a single broad tongue, with which a long cylindrical tube is associated. The reed end of the instrument is grasped by the lips, and by their pressure the slit between the reed and its frame is narrowed to the required extent. The overtones of a clarionet are different from those of a flute. A flute is an

FIG. 106.

open pipe, a clarionet a stopped one, the end at which the reed is placed answering to the closed end of the pipe. The tones of a flute follow the order of the natural numbers, 1, 2, 3, 4, &c., or of the even numbers, 2, 4, 6, 8, &c.; while the tones of a clarionet follow the order of the odd numbers, 1, 3, 5, 7, &c. The intermediate notes are supplied by opening the lateral orifices of the instrument. Sir C. Wheatstone was the first to make known this difference between the flute and clarionet, and his results agree with the more thorough investigations of Helmholtz. In the hautboy and bassoon we have two reeds inclined to each other at a sharp angle, with a slit between them, through which the air is urged. The pipe of the hautboy is *conical*, and its overtones are those of an open pipe—different, therefore, from those of a clarionet. The pulpy end of a straw of green corn may be split by squeezing it, so as to form a double reed of this kind, and such a straw yields a musical tone. In the horn, trumpet, and serpent, the performer's lips play the part of the reed. These instruments are formed of long conical tubes, and their overtones are those of an open organ pipe. The music of the older instruments of this class was limited to their overtones, the particular tone elicited depending on

the force of the blast and the tension of the lips. It is now usual to fill the gaps between the successive overtones by means of keys, which enable the performer to vary the length of the vibrating column of air.

§ 16. *The Voice.*

The most perfect of reed instruments is the organ of voice. The vocal organ in man is placed at the top of the trachea or windpipe, the head of which is adjusted for the attachment of certain elastic bands which almost close the aperture. When the air is forced from the lungs through the slit which separates these *vocal chords*, they are thrown into vibration; by varying their tension, the rate of vibration is varied, and the sound changed in pitch. The vibrations of the vocal chords are practically unaffected by the resonance of the mouth, though we shall afterwards learn that this resonance, by reinforcing one or the other of the tones of the vocal chords, influences in a striking manner the quality of the voice. The sweetness and smoothness of the voice depend on the perfect closure of the slit of the glottis at regular intervals during the vibration.

The vocal chords may be illuminated and viewed in a mirror placed suitably at the back of the mouth. Varied experiments of this kind have been executed by Sig. Garcia.[1] I once sought to project the larynx of M. Czermak upon a screen in this room, but with only partial success. The organ may, however, be viewed directly in the laryngoscope; its motions, in singing, speaking, and coughing, being strikingly visible. It is represented at rest in fig. 107. The roughness of the voice in colds is

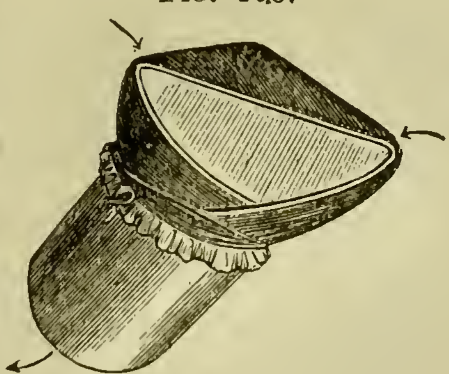
[1] I owe it to this eminent artist to direct attention to his experiments communicated to the Royal Society in May 1855, and recorded in the *Philosophical Magazine* for 1855, vol. x. p. 218.

due, according to Helmholtz, to mucous flocculi, which get into the slit of the glottis, and which are seen by means of the laryngoscope. The squeaking falsetto voice with which some persons are afflicted, Helmholtz thinks, may be produced by the drawing aside of the mucous layer which ordinarily lies under and loads the vocal chords. Their edges thus become sharper, and their weight less; while their elasticity remaining the same, they are shaken into more rapid tremors. The promptness and accuracy with which the vocal chords can change their tension, their form, and the width of the slit between them, to which must be added the elective resonance of the cavity of the mouth, renders the voice the most perfect of musical instruments.

Fig. 107.

The celebrated comparative anatomist, John Müller, imitated the action of the vocal chords by means of bands of india-rubber. He closed the open end of a glass tube by two strips of this substance, leaving a slit between them. On urging air through the slit, the bands were thrown into vibration, and a musical tone produced. Helmholtz recommends the form shown in fig. 108, where the tube, instead of ending in a section at right angles to its axis, terminates in two oblique sections, over which the bands of india-rubber are drawn. The easiest mode of obtaining sounds from reeds of this character is to roll round the end of a glass tube a strip of thin india-rubber, leav-

Fig. 108.



ing about an inch of the substance projecting beyond the end of the tube. Taking two opposite portions of the projecting rubber in the fingers, and stretching it, a slit is formed, the blowing through which produces a musical sound, which varies in pitch, as the sides of the slit vary in tension.

§ 17. *Vowel Sounds.*

The formation of the vowel sounds of the human voice excited long ago philosophic enquiry. We can distinguish one vowel sound from another, while assigning to both the same pitch and intensity. What, then, is the quality which renders the distinction possible? In the year 1769 this was made a prize question by the Academy of St. Petersburg, and Kratzenstein gained the prize for the successful manner in which, by mechanical arrangements, he imitated the vowel sounds. At the same time Von Kempelen, of Vienna, made similar and more elaborate experiments. The question was subsequently taken up by Mr. Willis, who succeeded beyond all his predecessors in the experimental treatment of the subject. The true theory of vowel sounds was first stated by Sir C. Wheatstone, and quite recently they have been made the subject of exhaustive enquiry by Helmholtz. You will find little difficulty in comprehending their origin.

Mounted on the acoustic bellows, without any pipe associated with it, this free reed speaks in a forcible manner. When upon the frame of the reed a pyramidal pipe is fixed, you notice a change in the sound; and by pushing my flat hand over the open end of the pipe, the similarity between the sound then produced and that of the human voice is unmistakable. Holding the palm of the hand over the end of the pipe so as to close it altogether, and then raising the hand twice in quick succession, the word 'mamma' is

heard as plainly as if it were uttered by an infant. For this pyramidal tube I now substitute a shorter one, and with it make the same experiment. The 'mamma' now heard is exactly such as would be uttered by a child with a stopped nose. Thus, by associating with a vibrating reed a suitable pipe, we can impart to the sound the qualities of the human voice.

In the organ of voice the reed is formed by the vocal chords, and associated with this reed is the resonant cavity of the mouth, which can so alter its shape as to resound, at will, either to the fundamental tone of the vocal chords or to any of their overtones. With the aid of the mouth, therefore, we can *mix together* the fundamental tone and the overtones of the voice in different proportions. Different vowel sounds are due to different admixtures of this kind. Striking one of this series of tuning-forks, and placing it before my mouth, I adjust the size of that cavity until it resounds forcibly to the fork. Then, without altering in the least the shape or size of my mouth, I urge air through the glottis. The vowel sound 'U' (*o o* in hoop) is produced, and no other. I strike another fork, and placing it in front of the mouth, adjust the cavity to resonance. Then removing the fork and urging air through the glottis, the vowel sound 'O,' and it only, is heard. I strike a third fork, adjust my mouth to it, and then urge the air outwards; the vowel sound *ah!* and no other, is heard. In all these cases the vocal chords have been in the same constant condition. They have generated throughout the same fundamental tone and the same overtones, the changes of sound which you have heard being due solely to the fact that different tones in the different cases were reinforced by the resonance of the mouth. Donders first proved that the mouth resounded differently for the different vowels.

In the formation of the different vowel sounds the resonant cavity of the mouth undergoes, according to Helmholtz, the following changes :—

For the production of the sound ‘ U ’ (*o o* in hoop), the lips must be pushed forward, so as to make the cavity of the mouth as deep as possible, and the orifice of the mouth, by the contraction of the lips, as small as posible. This arrangement corresponds to the deepest resonance of which the mouth is capable. The fundamental tone itself of the vocal chords is here reinforced, while the higher tones retreat.

The vowel ‘ O ’ requires a somewhat wider opening of the mouth. The overtones which lie in the neighbourhood of the middle *b* of the soprano come out strongly in the case of this vowel.

When ‘ Ah ’ is sounded, the mouth assumes the shape of a funnel widening outwards. It is thus tuned to a note an octave higher than in the case of the vowel ‘ O.’ Hence, in sounding ‘ Ah,’ those overtones are most strengthened which lie near the higher *b* of the soprano. As the mouth is in this case wide open, all the other overtones are also heard, though feebly.

In sounding ‘ A ’ and ‘ E,’ the hinder part of the mouth is deepened, while the front of the tongue rises against the gums and forms a tube; this yields a higher resonance-tone, rising gradually from ‘ A ’ to ‘ E,’ while the hinder hollow space yields a lower resonance-tone, which is deepest when ‘ E ’ is sounded.

These examples sufficiently illustrate the subject of vowel sounds. We may blend in various ways the elementary tints of the solar spectrum, producing innumerable composite colours by their admixture. Thus also may elementary sounds be blended so as to produce all possible varieties of clang-tint. After having resolved the human voice into its constituent tones, Helmholtz was able to

imitate these tones by tuning-forks, and, by combining them appropriately together, to produce the sounds of all the vowels.

§ 18. *Bell's Telephone.*

Let a bar of iron be surrounded near one of its ends with a coil of wire, overspun with silk or cotton, and let the two naked ends of the coil be brought into good metallic contact. If, under these circumstances, one pole of a bar magnet be moved towards the surrounded end of the iron bar, then, during the forward motion of the magnet, a current of electricity will run round the coil, and cease when the forward motion ceases.

If the end of the magnet be moved away from the coil, then, during the time of its removal, a current will be evoked in the coil flowing in a direction opposed to that of the first one.[1]

Reversing the position of things by placing a bar magnet, instead of the iron bar, within the coil, and a piece of iron, instead of the magnet, outside; on causing the iron to approach the end of the magnet and to retreat from it, two currents, opposed to each other in direction, will, as in the former case, be successively evoked. The quicker the to-and-fro motion, the quicker, of course, will be the generation and alternation of the currents.

A very slight motion suffices to produce such currents. Let one of the two prongs of a tuning-fork be placed in front of the bar magnet and coil; on causing the fork to sound by the passage of a bow, its vibrations, however small, will excite alternating currents in the adjacent coil, those excited by the approach of the prong being opposed in direction to those excited by its retreat.

[1] In a small shilling volume, published by Longmans, and entitled *Notes on Electricity*, the generation and character of these 'induced currents' are described.

If the currents thus evoked by a tuning-fork, instead of being confined to a single coil, be carried round a second one provided with a second bar magnet, then the attraction exerted by that magnet on the adjacent prong of a second tuning-fork, will be subject to alternations corresponding to the currents flowing round the magnet. The prong will be alternately pulled and released by the magnet; and if its period of vibration be the same as that of the fork which generates the currents, the second fork will sound in unison with the first. Through the intermediation of electric currents, the vibrations of the one fork are thus transmitted to the other.

Thin plates and membranes readily respond to sonorous vibrations. Sand, for example, strewn upon a stretched membrane is seen to shiver and dance when the voice acts upon the membrane. It is the capacity of the tympanic membrane to accept and transmit all the vibrations of the external air that enables us to hear the vast variety of sounds audible to us. If, therefore, a plate of iron of suitable thinness—a ferrotype plate, for example—be placed in front of one of our bar magnets, on projecting the voice against the plate, its vibrations will evoke currents like the tuning-fork, and these currents, transmitted to a second coil surrounding a second magnet, will reproduce, in a second thin plate of iron placed in front of the latter, all the vibrations impressed upon the first. Through the intermediation of electric currents spoken words may be thus transmitted. This is the principle of Bell's telephone.

Bell's telephone, therefore, depends on the capacity of a thin iron plate to take up the vibrations of the human voice; on the capacity of a magnet to respond by slight changes of its magnetism to the vibrations of such a plate; on the capacity of such changes to evoke electric currents corresponding to them in strength, direction, and duration; and, finally, on the capacity of these currents to be trans-

mitted to a distance, and there to reproduce vibrations exactly similar to those which gave the currents birth.

Prior to Mr. Bell the generation of the currents here referred to, and the laws of their action, were subjects of familiar knowledge. It was well known that a slight motion of a bit of iron in presence of a magnet suffices to excite such currents, and that they could be transmitted to a distance at will. But nobody prior to Mr. Bell had any notion that such currents could be generated by vibrations so minute and complicated as those of articulate speech, or that they could be compounded and transmitted so as to reproduce it.

§ 19. *Edison's Telephone.*

In the Edison telephone, sounds are also transmitted by the intermediation of electric currents, but these currents are generated and applied in a different manner. In 1879 the performance of the Edison telephone was illustrated in the theatre of the Royal Institution. Through the kindness of Lord John Manners and of the Post Office authorities, a wire, passing through the air from Albemarle Street to Piccadilly Circus, was placed at my service. The two ends of this wire being connected with the public water-pipes at the respective stations, a circuit was established through which a voltaic current could flow In the circuit, at each end of the air-wire, was placed an ordinary carbon telephone (to be referred to immediately), into which the messages were spoken. But while the receiver, at the Circus, of the messages sent from the Royal Institution, was a Bell's magnetic telephone held to the ear, the receiver at the Royal Institution was Edison's loud-speaking telephone The nephew of Mr. Edison, who bore his name, was stationed at the Circus, while Mr. Adams operated with the new instrument in Albemarle Street.

Passages from Shakspeare, Scott, Tennyson, Macaulay, and Burns, spoken by me through the carbon telephone, were received at the Circus, there repeated by Mr. Edison, and returned with an accuracy and loudness which enabled them to be heard throughout the theatre. Not only were selected phrases thus heard, but a poem of Emerson's was read out here from beginning to end, and sent back line by line with extraordinary fidelity and distinctness. Various expressions, moreover, following the quotations, such as 'Excellent!' 'Perfectly satisfactory!' 'Exceedingly good!' were promptly returned and heard with amusing intensity by the audience. Perhaps the most striking illustration of the pliant power of the instrument was its capability to reproduce a whistled tune. Mr. Edison's whistling at the Circus was heard in Albemarle Street almost as distinctly as if it had been produced upon the spot. After the lecture I quitted the theatre for a time, during which some members of the audience took my place. On my return I resumed the carbon telephone, and spoke into it. Mr. Edison immediately detected the difference of tone, and, on being asked who it was that now spoke, answered correctly. By this new instrument, therefore, the varying qualities of the human voice are in a remarkable degree reproduced.

These extraordinary effects were obtained with an apparatus so simple, and apparently so rude, that, without hearing the instrument, its alleged performance could hardly have been believed. I shall now endeavour in a familiar way to make clear both its construction and its action. Suppose the flat hand of an observer to be placed upon the surface of a rotating cylinder, the pressure being so regulated as to produce considerable friction between cylinder and hand. Let the direction of the rotation be such that the friction shall draw the observer towards the cylinder, he, at the same time, poising his body so as to

resist the pull. If the surface of the cylinder were uniformly smooth, a uniform frictional resistance would be experienced, the inclination of the observer's body remaining constant. But supposing different parts of the surface to be of different degrees of smoothness, varying suddenly from the slipperiness of ice to the roughness of cartridge-paper and felt, it is plain that on passing from the rough and adherent surface to the slippery one there would be a sudden relaxation of the friction. The force previously exerted to prevent the body from falling forwards would now cause it to fall backwards, until the hand had been again caught by another adherent portion of the surface. It is obvious that such a cylinder, rotating uniformly, would, in virtue of its alternate slipping and biting, compel the observer's body to vibrate to and fro.

In the Edison telephone there is a small rotating cylinder, and a flat strip of metal, one end of which is pressed down upon the cylinder by a spring. The other end of this metal rubber is attached to the centre of a thin circular plate of mica about four inches in diameter. The cylinder is formed of powdered chalk, with which are mixed a little hydrate of potash and acetate of mercury, the powder being squeezed to hardness by pressure in a cylindrical mould. Through the centre of the cylinder passes a metallic axis. This is connected with one end of the secondary wire of a very small induction coil, the other end of the wire being joined to the flat strip of metal above referred to. When moistened, the cylinder becomes to all intents and purposes an electrolyte, every passage of a current producing an amount of decomposition exactly proportionate to the current's strength and duration.

From a very small voltaic battery, a current was sent through the primary wire of the small induction coil, thence through the carbon telephone held by me, onward to Piccadilly Circus, from which it returned under the earth

to Albemarle Street. As long as this current flowed without any variation of strength, no effect whatever was produced upon the Edison telephone. A hand turning the crank of the chalk cylinder experienced a uniform resistance, the mica plate being drawn inwards with a constant force. In the carbon telephones employed in these experiments, a thin cake of fine petroleum lampblack was held between two thin plates of platinum, on one of which the voice impinged. The alternate compression and relaxation of the lampblack, by varying the resistance, produced variations in the voltaic current corresponding to the vibrations of the voice. Every variation thus introduced into the primary current started an induced current in the small secondary coil, while every such induced current produced its due amount of electro-chemical decomposition at the common surface of chalk cylinder and metallic rubber. By this decomposition a lubricant was liberated underneath the rubber, which immediately yielded, by slipping, to the tension of the mica plate. Each slip was of momentary duration, being followed by a frictional 'bite' which drew the mica diaphragm inwards as before. Thus the vibrations of the voice—of its tones and overtones—were in the first place impressed upon the primary current, every variation of the latter being followed by a proportionate discharge of secondary currents through the induction coil. By their electrolytic action these induced discharges produced and controlled the slipping of the metal rubber, causing it to vibrate longitudinally in accordance with the vocal vibrations. These were finally transferred, with their qualities to a surprising extent intact, to the plate of mica, and thence to the surrounding air. The mica plate might, therefore, be regarded as a magnified tympanic membrane, the latter, like the plate, being drawn inwards by the bones of the ear. It may seem amazing that the mica should be

able to take up and reproduce with such intensity and distinctness the manifold vibrations involved in whistling and speaking; but the wonder was anticipated by an artificer more ancient than Mr. Edison, in the construction of the tympanum itself.

The germinal observation, if we may use the term, from which the loud-speaking telephone has sprung, was, we believe, made by Mr. Edison in 1872, while experimenting on moist papers with a view to telegraphic applications. He then noticed the slipping by electrolysis which he has recently turned to such excellent account. The lubricant is probably hydrogen gas.

§ 20. *The Microphone.*

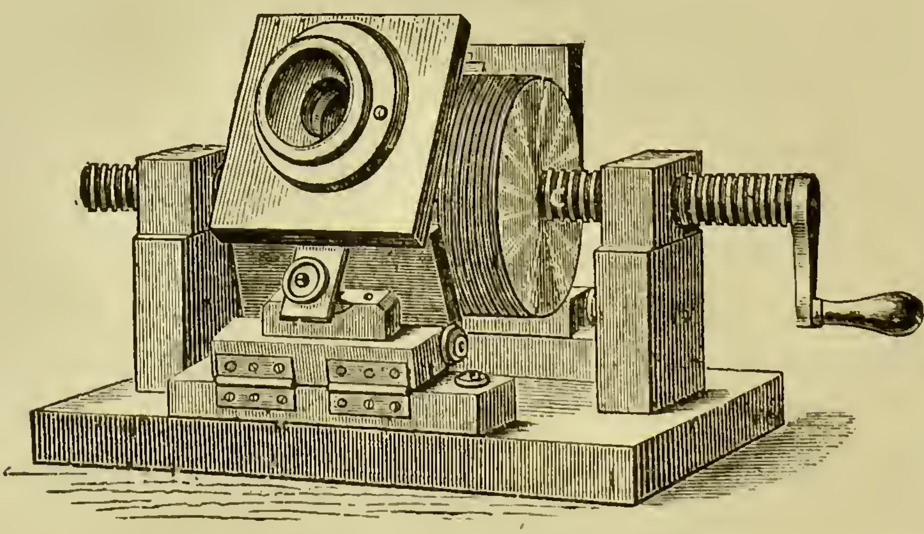
So far as it depends on the rapid changes of a voltaic current produced by variations of resistance, the principle of the microphone is the same as that of the carbon telephone. The microphone, however, is a different instrument, and is applied to different ends. It is the invention of Professor Hughes. Let a spindle of carbon pointed at both ends be set upright, resting below in a cup of carbon and prevented from falling by an inverted cup of carbon at the top. The spindle must rest loosely in the cups, so as to be sensitive to the slightest shake, whether produced acoustically or mechanically. The two carbon cups which sustain the spindle are connected with the two poles of a small voltaic battery, with a telephone in its circuit. The current passes from one pole of the battery through the carbon spindle, returning thence through the telephone to the other pole. As long as no change occurs in the contact of the carbons, the current remains constant, and nothing is heard at the telephone. But the slightest shaking of the upright spindle alters the contact, and affects the current, the changes of which imme-

diately announce themselves upon the telephone. The shaking may be produced by the inaudible rolling of carriages in the street, by the voice, by the passage of a brush, or even by the walking of a heavy fly. Hence the name of the instrument. It must not be supposed that there is any magnifying of the sounds, as such; the action is entirely due to the mechanical shaking of the carbons, which, by changing the strength of the current, changes correspondingly the force of the telephone magnet, causing it, by the varying strength of its attraction, to excite tremors in the thin plate of iron which, in Bell's telephone, is the proximate source of the sound.

§ 21. *The Phonograph.*

We owe this instrument to the inventive genius of Mr. Edison. Fig. 109 represents a perspective view of the phonograph, and fig. 110 is a sectional diagram.

FIG. 109.

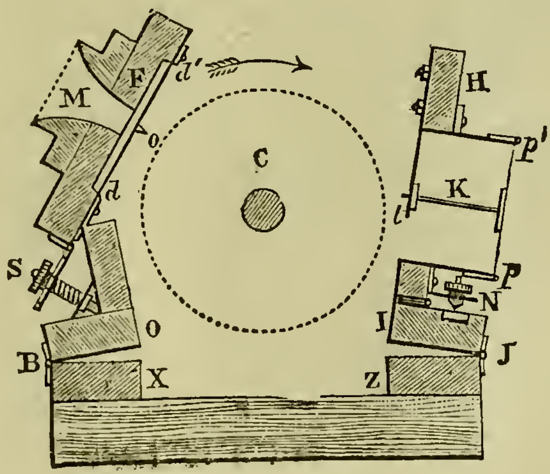


C is a cylinder of brass having a spiral groove or screw cut upon its surface from end to end. It is mounted upon a spindle (well seen in fig. 109), whose length is rather more than three times that of the cylinder. At one end of the spindle is a winch handle, between which and the cylinder

a screw is cut of the same pitch as that upon the cylinder —viz. eight threads to the inch. The spindle turns in two brass bearings, one of which has an inside screw corresponding to the screw upon the spindle.

A very thin sheet-iron diaphragm $d\ d'$ (fig. 110) receives the operator's voice. It is secured by means of a brass flange over a shallow circular recess cut in the wooden frame F. In the middle of the recess is a round hole opening into the mouthpiece M. A short piece of hard steel wire, ground to a blunt point and carefully polished, is fixed perpendicularly to the iron diaphragm at its centre o.

Fig. 110.



On the other side of the cylinder is the diaphragm $p\ p'$, which reproduces the sound of the voice. It is made of vegetable parchment, and is stretched like a drum over the end of a piece of brass tube 1 inch long and 2¾ inches in diameter, fixed in the wooden frame H. The steel point t is attached to the end of a steel spring, its position coinciding with the centre of the circular opening in H. The light pine rod K, connecting the parchment and the spring, abuts at the end next to the spring against a small pad of vulcanised india-rubber, and at the other end carries a disc of thin sheet iron half an inch in diameter. The pressure of the spring is sufficient to render the diaphragm very slightly convex.

The square frame F, carrying the thin iron diaphragm which receives the voice, is attached by hinges to a piece of wood, which is connected rigidly with O, O again being connected by hinges B with X. By means of these hinges, and the screw S, the point o may be made to approach the

cylinder, so as to dip into the groove upon its surface. The arrangement on the other side of the cylinder is almost identical. At I and J are hinges and at N a screw-nut and spring for regulating the depth to which *t* enters the groove.

The method of using the instrument is as follows. The two diaphragms being turned back, the cylinder is covered with a piece of stout tinfoil, fixed by gum, and is then moved, by turning the handle, as far to the right as possible. The diaphragms are restored to the position shown in fig. 110: the stud at O is turned so as to allow the point *o* to press upon the tinfoil, and the cylinder is made to perform half a revolution. *d d′* is once more raised, and if the smooth furrow formed by the motion of the tinfoil against the point should be found too deep or too shallow, the nut S is turned slightly to the right or left. When the proper depth is attained—a point only to be learnt by experience—the same process is gone through with the other diaphragm, and the instrument is in working order. The point *o* is again dropped upon the tin-foil, the diaphragm *p p′* being kept raised as in the figure. The handle is then regularly turned at the rate of about one revolution in a second, words being at the same time distinctly spoken into the mouthpiece M. When the speech is completed, *d d′* is removed from the tinfoil, when the furrow it has traced is found indented by the movements of the point in responding to the vibrations of the voice. The cylinder is then turned back to its original position, and the point *t*, at the other side of the cylinder, is dropped upon the furrow. Again the handle is turned, the little undulations on the foil being caused to pass under the point. The original process is thus reversed, the indentations imparting to the second point movements precisely similar to those which the first point performed while producing the indentations. The vibrations of *t* are

conveyed by the rod K to the parchment diaphragm $p\ p'$, and by it to the air, producing sounds closely approximating in pitch and quality to those by which the iron diaphragm was set in motion. A cone of stout drawing-paper about a foot long should be slipped over the tube $p\ p'$ to reinforce and give body to the sound.

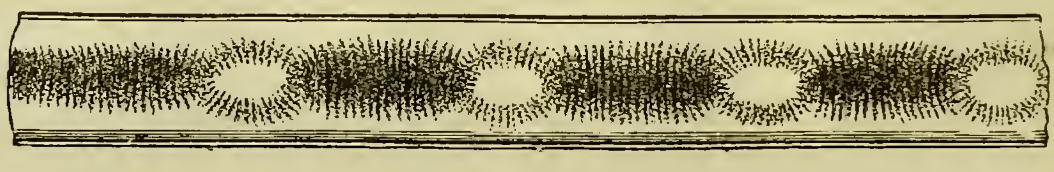
A single diaphragm might be used both for the production of the indentations and the reproduction of the sound; and, indeed, many instruments have been constructed on that principle. But the arrangement just described is found to give the best results. I am indebted for the description to Mr. Shelford Bidwell.

§ 22. *Kundt's Experiments: new modes of determining Velocity of Sound.*

You have already heard the tones, and made yourselves acquainted with the various modes of division of a glass tube, free at both ends, when thrown into longitudinal vibration. When it sounds its fundamental tone, you know that the two halves of such a tube lengthen and shorten in quick alternation. If the tube were stopped at its ends, the closed extremities would throw the air within the tube into a state of vibration; and if the velocity of sound in air were equal to its velocity in glass, the air of the tube would vibrate in synchronism with the tube itself. But the velocity of sound in air is far less than its velocity in glass, and hence, if the column of air is to synchronise with the vibrations of the tube, it can only do so by dividing itself into vibrating segments of a suitable length. In an investigation of great interest published in Poggendorff's 'Annalen,' Professor Kundt has taught us how these segments may be rendered visible. Into this six-foot tube is introduced the light powder of lycopodium, which is shaken all over the interior surface. A

small quantity of the powder clings to that surface. Stopping the ends of the tube, holding its centre by a fixed clamp, and sweeping a wet cloth briskly over one of its halves, instantly the powder, which a moment ago clung to its interior surface, falls to the bottom of the tube in the forms shown in fig. 111, the arrangement of the

Fig. 111.



lycopodium marking the manner in which the column of air has been divided. Every node here is encircled by a ring of dust, while from node to node the dust arranges itself in transverse streaks along the ventral segments.

You will have little difficulty in seeing that we perform here, with air, substantially the same experiment as that of M. Melde with a vibrating string. When the string was too long to vibrate as a whole, it met the requirements of the tuning-fork to which it was attached by dividing into ventral segments. Now, in all cases, the length from a node to its next neighbour is half that of the sonorous wave: how many such half-waves, then, have we in our tube in the present instance? Sixteen (the figure shows only four of them). But the length of our glass tube vibrating thus longitudinally is also half that of the sonorous wave *in glass*. Hence, in the case before us, with the same rate of vibration, the length of the semi-wave in glass is sixteen times the length of the semi-wave in air. In other words, the velocity of sound in glass is sixteen times its velocity in air. Thus, by a single sweep of the wet rubber, we solve a most important problem. But, as M. Kundt has shown, we need not confine ourselves to air. Introducing any other gas into the tube, a single stroke of

our wet cloth enables us to determine the relative velocity of sound in that gas and in glass. When hydrogen is introduced, the number of ventral segments is less than in air; when carbonic acid is introduced, the number is greater.

From the known velocity of sound in air, coupled with the length of one of these dust segments, we can immediately deduce the number of vibrations executed in a second by the tube itself. Clasping a glass tube at its centre and drawing my wetted cloth over one of its halves, I elicit this shrill note. The length of every dust segment, now within the tube, is 3 inches. Hence the length of the aerial sonorous wave corresponding to this note is 6 inches. But the velocity of sound in air of our present temperature is 1,120 feet per second; a distance which would embrace 2,240 of our sonorous waves. This number, therefore, expresses the number of vibrations per second executed by the glass tube now before us.

Instead of damping the centre of the tube, and making it a nodal point, we may employ any other of its subdivisions. Laying hold of it, for example, at a point midway between its centre and one of its ends, and rubbing it properly, it divides into three vibrating parts, separated by two nodes. We know that in this division the note elicited is the octave of that heard when a single node is formed at the middle of the tube; for the vibrations are twice as rapid. If, therefore, we divide the tube, having air within it, by two nodes instead of one, the number of ventral segments revealed by the lycopodium dust will be thirty-two instead of sixteen. The same remark applies, of course, to all other gases.

Filling a series of four tubes with air, carbonic acid, coal gas, and hydrogen, and then rubbing each so as to produce two nodes, M. Kundt found the number of dust

segments formed within the tube in the respective cases to be as follows:—

Air	32 dust segments.
Carbonic acid . .	40	,,
Coal gas . . .	20	,,
Hydrogen . . .	9	,,

Calling the velocity in air unity, the following fractions express the ratio of this velocity to those in the other gases :—

Carbonic acid . .	$\frac{32}{40} = 0{\cdot}8$
Coal gas . . .	$\frac{32}{20} = 1{\cdot}6$
Hydrogen . . .	$\frac{32}{9} = 3{\cdot}56$

Referring to a table introduced in our first chapter, we learn that Dulong by a totally different mode of experiment found the velocity in carbonic acid to be 0·86, and in hydrogen 3·8 times the velocity in air. The results of Dulong were deduced from the sounds of organ pipes filled with the various gases ; but here, by a process of the utmost simplicity, we arrive at a close approximation to his results. Dusting the interior surfaces of our tubes, filling them with the proper gases, and sealing their ends, they may be preserved for an indefinite time. By properly shaking one of them at any moment, its inner surface becomes thinly coated with the dust; and afterwards a single stroke of the wet cloth produces the division from which the velocity of sound in the gas may be immediately inferred.

Savart found that a spiral nodal line is formed round a tube or rod when it vibrates longitudinally, and Seebeck showed that this line was produced, not by longitudinal, but by secondary transverse vibrations. Now, this spiral nodal line tends to complicate the division of the dust in our present experiments. It is, therefore, desirable to operate

in a manner which shall altogether avoid the formation of this line; M. Kundt has accomplished this, by exciting the longitudinal vibrations in one tube, and producing the division into ventral segments in another, into which the first fits like a piston. Before you is a tube of glass, fig. 112, seven feet long, and two inches internal diameter. One end of this tube is stopped by the movable cork, *b*. The other end, K K, is also stopped by a cork, through the centre of which passes the narrower tube, A *a*, which is firmly clasped at its middle by the cork, K K. The end of the interior tube is also closed with a projecting stopper, *a*, almost sufficient to fill the larger tube, but still fitting into it so loosely that the friction of *a* against the interior surface is too slight to interfere practically with its vibrations. The interior surface between *a* and *b* being lightly coated with the lycopodium dust, a wet cloth is passed briskly over A K; instantly the dust between *a* and *b* divides into a number of ventral segments. When the length of the column of air, *a b*, is equal to that of the glass tube, A *a*, the number of ventral segments is sixteen. When, as in the figure, *a b* is only one-half the length of A *a*, the number of ventral segments is eight.

Fig. 112.

But here you must perceive that the method of experiment is capable of great extension. Instead of the glass tube, A *a*, we may employ a rod of any other solid substance—of wood or metal, for example, and thus determine the relative velocity of sound in the solid and in air. In the place of the glass tube, for example, a rod of brass of equal length may be employed.

Rubbing its external half by a resined cloth, it divides the column *a b* into the number of ventral segments proper to the metal's rate of vibration. In this way M. Kundt operated with brass, steel, glass, and copper, and his results prove the method to be capable of great accuracy. Calling, as before, the velocity of sound in air unity, the following numbers expressing the ratio of the velocity of sound in brass to its velocity in air were obtained in three different series of experiments :—

1st experiment	10·87
2nd experiment	10·87
3rd experiment	10·86

The coincidence is here extraordinary. To illustrate the possible accuracy of the method, the length of the individual dust segments was measured. In a series of twenty-seven experiments, this length was found to vary between 43 and 44 millimètres (each millimètre $\frac{1}{25}$th of an inch), never rising so high as the latter, and never falling so low as the former. The length of the metal rod, compared with that of one of the segments capable of this accurate measurement, gives us at once the velocity of sound in the metal, as compared with its velocity in air.

Three distinct experiments, performed in the same manner on steel, gave the following velocities, the velocity through air, as before, being regarded as unity :—

1st experiment	15·34
2nd experiment	15·33
3rd experiment	15·34

Here the coincidence is quite as perfect as in the case of brass.

In glass, by this new mode of experiment, the velocity was found to be

15·25.[1]

[1] The velocity in glass varies with the quality; the result of each experiment has therefore reference only to the particular kind of glass employed in the experiment.

Finally, in copper the velocity was found to be

11·96.

These results agree extremely well with those obtained by other methods. Wertheim, for example, found the velocity of sound in steel wire to be 15·108; M. Kundt finds it to be 15·34: Wertheim also found the velocity in copper to be 11·17; M. Kundt finds it to be 11·96. The differences are not greater than might be produced by differences in the materials employed by the two experimenters.

The length of the aerial column may or may not be an exact multiple of the wave-length, corresponding to the rod's rate of vibration. If not, the dust segments usually take the form shown in fig. 113. But if, by means of the

FIG. 113.

stopper, *b*, the column of air be made an exact multiple of the wave-length, then the dust quits the vibrating seg-

FIG. 114.

ments altogether, and forms, as in fig. 114, little isolated heaps at the nodes.

§ 23. *Explanation of a Difficulty.*

And here a difficulty presents itself. The stopped end *b* of the tube fig. 112 is, of course, a place of no vibration, where in all cases a nodal dust-heap is formed; but whenever the column of air was an exact multiple of the wave-length, M. Kundt always found a dust-heap close to the

end *a* of the vibrating rod also. Thus the point from which all the vibration emanated seemed itself to be a place of no vibration.

This was pointed out by M. Kundt as a difficulty, but he did not attempt its solution. We are now in a condition to explain it. In Lecture III. it was remarked that in strictness a node is not a place of no vibration; that it is a place of *minimum* vibration; and that by the addition of the minute pulses which the node permits, vibrations of vast amplitude may be produced. The ends of M. Kundt's tube are such points of minimum motion, the lengths of the vibrating segments being such that, by the coalescence of direct and reflected pulses, the air at a distance of half a ventral segment from the end of the tube vibrates much more vigorously than that at the end of the tube itself. This addition of impulses is most perfect when the aerial column is an exact multiple of the wave-length, and hence it is that, in this case, the vibrations become sufficiently intense to sweep the dust altogether away from the vibrating segments. M. Melde's tuning-forks, though the sources of all the motion, are nodes. The vibrating reed of a clarionet is also a node.

An experiment of Helmholtz's is here capable of instructive application. Upon the string of the sonometer described in our third lecture I place the iron stem of this tuning-fork, which executes 512 complete vibrations in a second. At present you hear no augmentation of the sound of the fork; the string remains quiescent. But moving the fork along the string, on reaching the number 33, a loud swelling note issues from the string. At this particular tension the length 33 exactly synchronises with the vibrations of the fork. By the intermediation of the string, therefore, the fork is enabled to transfer its motion to the sonometer, and through it to the air. The sound continues as long as the fork vibrates, but the least move-

ment to the right or to the left from this point causes a sudden fall of the sound. Tightening the string the note disappears; for it requires a greater length of this more highly tensioned string to respond to the fork. But on moving the fork further away, at the number 36 the note again bursts forth. Tightening still more, 40 is found to be the point of maximum power. When the string is slackened it must, of course, be shortened in order to make it respond to the fork. Moving the fork now towards the end of the string, at the number 25 the note is found as before. Again, shifting the fork to 35, nothing is heard: but by the cautious turning of the key the point of synchronism, if I may use the term, is moved further from the end of the string. It finally reaches the fork, and at that moment a clear full note issues from the sonometer. In all cases, before the exact point is attained, and immediately in its vicinity, we hear 'beats,' which, as we shall afterwards understand, are due to the coalescence of the sound of the fork with that of the string, when they are nearly, but not quite, in unison with each other.

In these experiments, though the fork was the source of all the motion, *the point on which it rested was a nodal point.* It constituted the comparatively fixed extremity of the wire whose vibrations synchronised with those of the fork. The case is exactly analogous to that of the hand holding the india-rubber tube, and to the tuning-fork in the experiments of M. Melde. It is also an effect precisely the same in kind as that observed by M. Kundt, where the part of the column of air in contact with the end of his vibrating rod proved to be a node instead of the middle of a ventral segment.

§ 24. *Conversion of Radiant Heat into Sound.*

I have now to introduce to your notice a series of experiments wherein musical sounds are produced in a

novel and instructive manner, and which at the same time throw light on important problems in molecular physics.

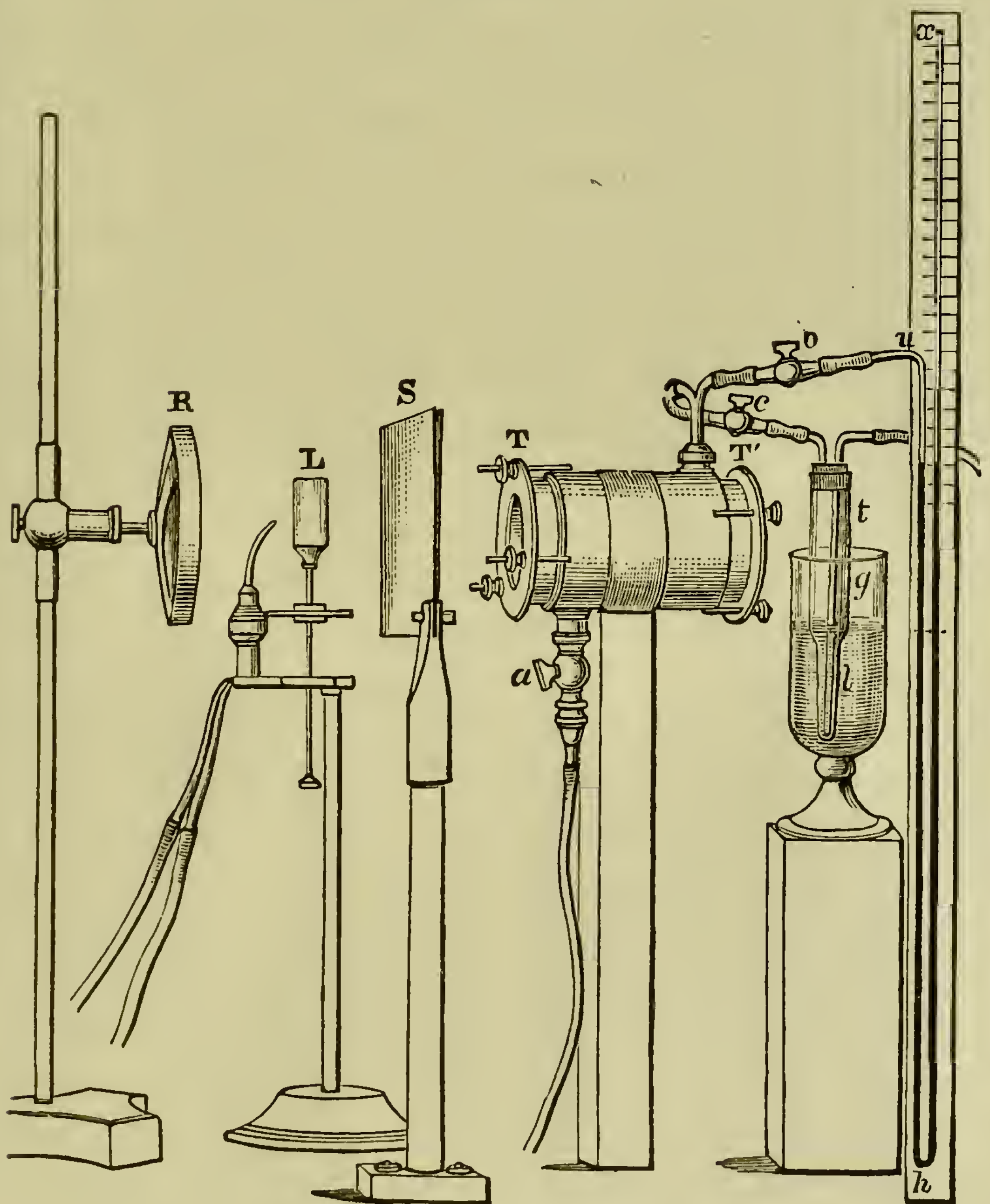
Professor Graham Bell was the first to show that an intermittent beam of *light*, impinging upon disks of solid matter, produced feeble but distinct musical sounds. When I first saw these experiments I came to the conclusion that the sounds were caused by the rhythmic absorption and emission of radiant heat.

Bodies absorb heat in different degrees. The rays from a common fire, for example, pass with much greater facility through rocksalt than through glass. In consequence of this the glass is heated by the fire, while the salt is not heated. The term diathermanous is used to express transparency to radiant heat, and rocksalt is therefore said to be more diathermanous than glass. It is not an unfrequent experience with us here to find the glass lenses of our electric lamps cracked by the heat absorbed by the lenses. Were the lenses of rocksalt, no such cracking would occur.

Similar differences, as regards the absorption of radiant heat, are found to exist in gaseous and vaporous bodies. Some of them are highly diathermanous, others are highly adiathermanous, or opaque to radiant heat. A perfectly diathermanous gas would allow radiant heat to pass through it without any change of its temperature. Common air, for example, is almost perfectly diathermanous, and through this medium, when dry, the most powerful heat-beams can pass without warming the air. When a gas is warmed it expands, and when it is cooled it contracts. If then a gas be exposed to an intermittent beam, which, when it reaches the gas, causes the latter to expand, and which, when it is intercepted, enables the gas to contract, such expansions and contractions, if they succeed each other with sufficient rapidity, may be

expected to produce sonorous pulses or musical sounds If, moreover, the magnitude of the expansion be determined by the amount of radiant heat absorbed, the sounding power of gases may be taken as a test of their absorbing power as regards radiant heat.

Fig. 115.



That gases absorb radiant heat, and suffer consequent thermal expansion in very different degrees, may be readily shown by the apparatus now to be described. T T,′ fig. 115, is a glass tube 4 inches long and 3 inches in

diameter. It is provided with brass flanges at the ends which reduce the diameter to 2·5 inches. Against these flanges, transparent plates of rocksalt were fixed air-tight. The tightness of the tube was secured, sometimes by india-rubber washers properly greased, and sometimes by cement. A stop-cock *a* near one end of T T′ was connected with a barometer-tube and an air-pump. A T-piece at the other end was connected by the one arm with a test-tube *t*, plunged in water contained in the glass *g*. The test-tube was connected with a purifying apparatus (not shown), consisting of two U-tubes, one containing fragments of Carrara marble wetted with caustic potash, the other containing fragments of glass wetted with sulphuric acid. Before entering these U-tubes the air was freed from suspended matter by a plug of cotton-wool. The other arm of the T-piece was connected with a quill tube of glass, *u h x*, bent into the shape of a U, the two legs of which contained a coloured liquid. The liquid column when standing at the same level in both arms of the U was 350 millimètres high in each, while the free leg of the U (shortened in the figure) rose to a height of about 500 millimètres above the surface of the liquid. The source of heat was the cylinder of lime L, rendered incandescent by a flame of coal gas and oxygen. The rays from the lime cylinder were received by a concave mirror R silvered in front, and sent by it in a convergent beam through the manometer-tube T T′. The focus of the beam was within the tube and near its most distant end. The gas and oxygen were supplied from gasholders specially constructed for these and similar experiments; long and futile experience of gas from the public mains, or compressed in iron bottles, having shown independent gasholders, which could be kept at an unalterable pressure, to be essential.

The experiments were conducted thus:—The test-tube *t*

(fig. 115) contained the liquid whose vapour was to be examined. Through a cork which stopped the test-tube passed a narrow tube of glass, ending in a small orifice near the bottom of the test-tube, and at a considerable depth below the surface of the liquid. To augment this depth, and to economise the liquid, the lower half of the test-tube was drawn out as shown in the figure. A second narrow tube passed also air-tight through the cork, and ended immediately beneath it. Both tubes were bent at a right angle above the cork. The manometric tube T T′ being exhausted, by turning the cock *c* air freed from its carbonic acid, its moisture, and its suspended matter, was allowed to bubble through the liquid in the test-tube *t*, and to pass thence into the manometric tube. To spare the oxygen in the gasholder, it was cut off during the interval between two consecutive experiments, the coal gas being kept continually alight. When the manometric tube was filled, the filling being always accomplished through an orifice of fixed dimensions, the oxygen was turned on, and the cylinder was allowed to remain for one minute under the action of the intensified flame. During this time a double silver screen S intercepted the radiation. At the end of a minute this screen was withdrawn, the beam then passing through the mixed air and vapour. The liquid in the adjacent leg of the narrow U-tube was immediately depressed, that in the opposite leg being equally elevated. The rise of this latter column above its starting point, marked zero on a millimètre scale, was accurately measured. Double this rise gave the difference of level in the two legs of the U, and this 'water pressure' expressed the augmentation of elastic force by the absorption of radiant heat.[1]

Here follow a few of the measurements which have been thus made.

[1] The method here applied and extended was introduced by Professor Röntgen.

Vapours.

Increase of Elastic Force by Radiant Heat.

	Name of liquid.	Water pressure.	Character of sound.
1.	Sulphuric Ether . .	300 millims.	Very strong
2.	Hydride of Amyl . .	279 „	„
3.	Acetone . . .	267 „	„
4.	Formic Ether . .	261 „	„
5.	Acetic Ether. . .	248 „	„
6.	Butyric Ether . .	183 „	Strong
7.	Formic Acid . . .	180 „	„
8.	Valeral	172 „	„
9.	Valerianic Ether . .	168 „	„
10.	Acetate of Propyl . .	166 „	„
11.	Benzol	117 „	Moderate
12.	Carbonic Ether . .	108 „	„
13.	Iodide of Allyl . .	92 „	„
14.	Chloroform . . .	89 „	„
15.	Bisulphide of Carbon .	81 „	„
16.	Cyanide of Methyl .	64 „	Weak
17.	Tetrachloride of Carbon .	58 „	„
18.	Xylol	44 „	„
19.	Amylic Alcohol . .	42 „	„
20.	Iodide of Amyl . .	42 „	„

The absorptive power of gases was determined by the same instrument with the following results:—

Gases.

Name of gas.	Water pressure.
Chloride of Methyl	350 millims.
Aldehyde	325 „
Olefiant Gas	315 „
Sulphuric Ether	300 „
Nitrous Oxide	198 „
Marsh Gas	164 „
Carbonic Acid	144 „
Carbonic Oxide	116 „
Oxygen.	5 „
Hydrogen	5 „
Nitrogen	5 „
Dry air	5 „
Humid air at 50° C.	130 „

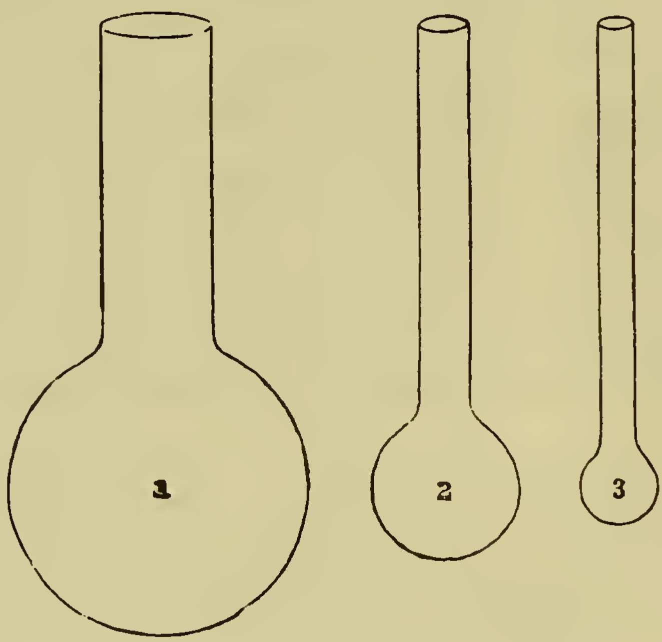
Sulphuric ether is here inserted with the view of connecting this Table with the last. Of all gaseous bodies hitherto examined, chloride of methyl is the most energetic absorber and the most powerful sound-producer; while dry air, oxygen, hydrogen, and nitrogen remained practically silent under the action of the most powerful beams. After chloride of methyl comes aldehyde, with a boiling point of 21° C. The figure 5 attached to the elementary gases, and to dry air, expresses, not absorption of radiant heat, but expansion, due to contact with the slightly warmed apparatus. The nitrous oxide employed was derived from an iron bottle in which it was preserved for medical purposes. In some of my experiments marsh gas showed itself a better absorber than nitrous oxide. This, for instance, was the case in experiments made in the spring of 1880 with the manometer. The sample of marsh gas wherewith the foregoing result was obtained was very carefully prepared in our chemical laboratory.

The temperature of 50° C. in the case of humid air was obtained in a wooden shed erected in our laboratory. The shed is traversed by two tubes of sheet iron 4 inches in diameter, which carry the heated air and products of combustion from two large ring-burners. It is 8′ 6″ long, 4′ 3″ wide, and 7″ high. The temperature of the air within it can be readily raised to 60° C. In the experiment above recorded the air was taken from the outside laboratory through a tube passing through the wooden wall of the shed. It was caused to bubble through water contained in a large flask which had been permitted to remain for some time in the warm shed. The mixed air and vapour entered the manometer-tube at a temperature some degrees lower than that of the tube itself. Closely examined, all parts of this tube were bright and dry when the vapour-laden air was within it. On per-

mitting the beam from the lime light (produced by coal gas and oxygen) to pass through the mixture, a prompt rise of 65 millimètres was the consequence. Cutting the beam off, the column rapidly returned to zero. The double of 65, or 130 millimètres, gives the difference of level in the two legs of the U-tube.

From these vapours and gases musical sounds were extracted in the following manner:—A little of the volatile

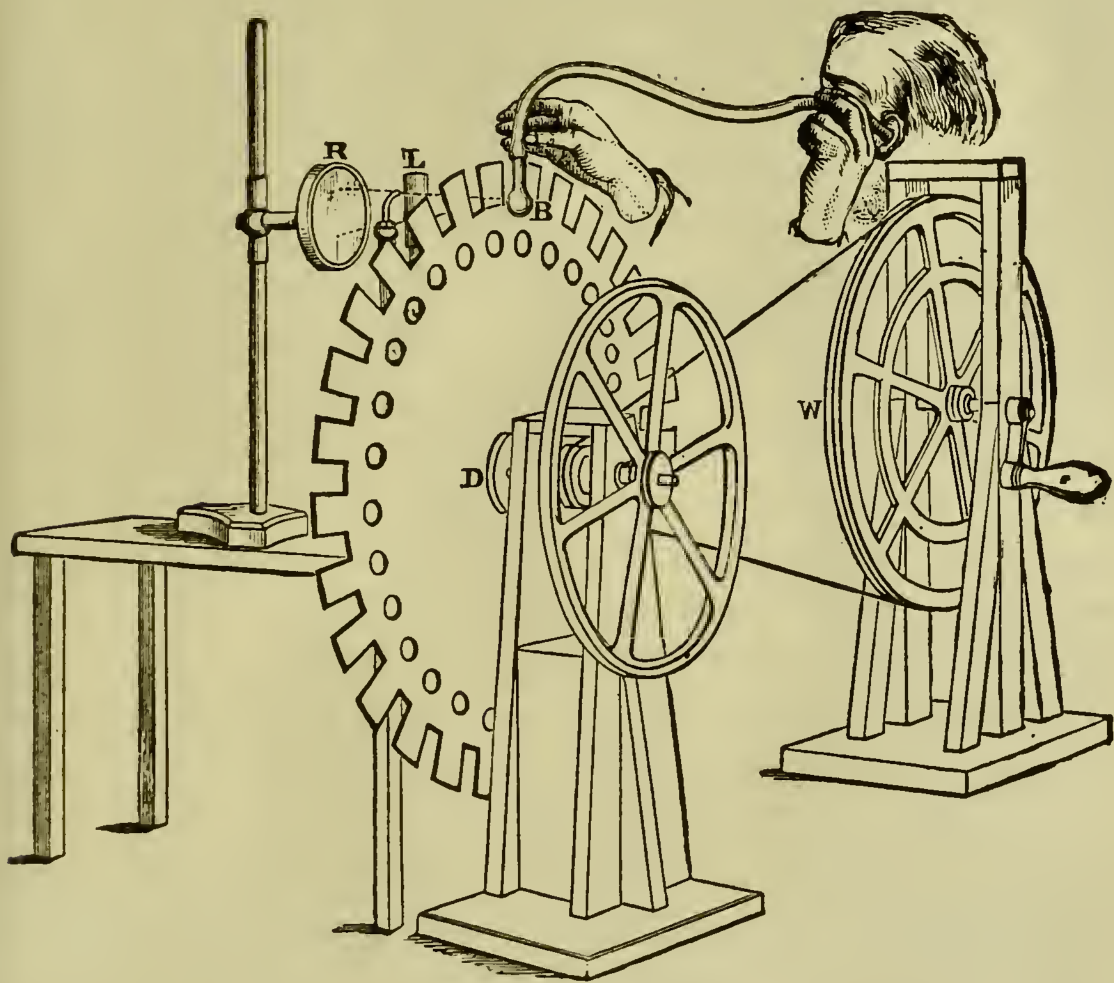
Fig. 116.



liquid being placed in a small flask, the vapour of the liquid was suffered to diffuse till the air of the flask above the liquid was saturated. Flasks or bulbs of the size shown in fig. 116, 1, 2, 3, were found very effective. To produce the required intermittence I first employed a circle of sheet zinc 16 inches in diameter provided with radial slits. This was afterwards exchanged for a second disc of the same diameter but furnished with circumferential teeth and interspaces. The disc was mounted vertically on a whirling table, and caused to rotate across the beam near the focus of the concave mirror. Immediately behind the disc was placed the flask containing the gas or

vapour to be examined, while an india-rubber tube, ending in a hollow cone of ivory or boxwood, connected the flask with the ear. With this arrangement, simple as it is, sounds of surprising intensity were obtained with all those gases and vapours which my previous experiments with an experimental tube and thermopile had

FIG. 117.



proved to be powerful absorbers of radiant heat. The final arrangement was that shown in fig. 117.

The source of heat is the carefully worked and centred lime-cylinder L, heated by the oxy-hydrogen flame. The rays from this source are received by the concave mirror R, and converged upon the bulb B, which contains the substance to be tested. The bulb is connected with the ear by a piece of india-rubber tubing, ending in a tapering

tube of boxwood or ivory. The intermittence of the calorific beam is effected by the disc D of strong card-boarfl, 2 feet in diameter, and provided at the circumference with 29 teeth and corresponding interspaces.[1] The disc is caused to rotate by the wheel W, with which it is connected by a band. The positions of the sonorous bulb and of its ear-tube are shown in the figure. In the case of gases lighter than air the bulb B is turned upside down. With the heavier gases it is held erect. When vapours are tested a small quantity of liquid is poured into the bulb, which is shaken so as to diffuse the vapour in the air above the liquid. The bulb is so held that the point of maximum concentration of the beam falls upon it.

With this apparatus I have tested more than once the sounding power of ten gases and of about eighty vapours. As a sound-producer chloride of methyl is supreme. It is, however, closely followed by aldehyde, olefiant gas and sulphuric ether, the two latter being very nearly equal to each other. The volatility of the liquid from which the vapour is derived is of course an important factor in the result. For, however high the inherent capacity of the molecule as an absorber may be, if the molecules be scanty in number the effect is small. Feeble vapours may to some extent atone, by quantity, for the individual weakness of their molecules. A few examples will, however, suffice to show that the specific action of the molecules over-rides sometimes the effect of volatility. Bisulphide of carbon with a boiling point of 42° C. is less powerful than acetic ether with a boiling point of 74°. Tetrachloride of carbon boils at 77°, but its sound by no means equals that of acetal which boils at 104°. Chloroform with a boiling point of 61° is less powerful as a sound producer than valeral with a boiling point of 100°,

[1] Intermittence is sometimes produced by the series of equidistant circular apertures shown in the figure.

or even than valerianic ether with a boiling point of 144°. Cyanide of methyl boils at 82°, but produces less sound than acetate of propyl with a boiling point of 102°. The mere inspection of the foregoing table of vapours will show how the intensity of the sound varies with the absorbent power of the vapour, changing from 'very strong' in the case of sulphuric ether, with a water pressure of 300 millimètres; through 'strong,' 'moderate,' and 'weak;' down to iodide of amyl, the expansion of which, by radiant heat, is only able to produce a water pressure of 42 millimètres.

The universality of its presence, and the discussions which it has provoked, rendered the action of water vapour especially interesting to me. I did not imagine at the outset that the modicum of vapour diffused in atmospheric air at ordinary temperatures could produce sonorous pulses of sensible intensity. In my first experiment, therefore, I warmed water in a flask nearly to its boiling point. I heated the flask above the water with a spirit-lamp flame, thus dissipating every trace of haze, and then exposed the clear vapour to the intermittent beam. The experiment was a virtual question put to the vapour as to whether I had been right or wrong in ascribing to it the power of absorbing radiant heat. The vapour answered by emitting a musical note which, when properly converged upon the tympanum, seemed as loud as the peal of an organ. When the temperature was lowered from 100° C. to 10° C. the sound did not vanish, as I expected it would. It remained not only distinct but strong. The flasks employed in these experiments were dried in a variety of ways, of which I have elsewhere given some account, and which will suggest themselves to every experimenter in this field. Taken open from the laboratory, and exposed to the intermittent beam, the flasks are always to some extent sonorous.

Placed beside sulphuric acid underneath the receiver of an air-pump, and permitted to dry there, they are reduced to silence. The slightest invasion of humid air renders them again sonorous. Breathing for a moment into a dried and silent flask, a loud sounding power is immediately manifested.

SUMMARY OF LECTURE V.

When a stretched wire is suitably rubbed, in the direction of its length, it is thrown into longitudinal vibration: the wire can either vibrate as a whole or divide itself into vibrating segments separated from each other by nodes.

The tones of such a wire follow the order of the numbers, 1 2, 3, 4, &c.

The *transverse* vibrations of a rod fixed at both ends do not follow the same order as the transverse vibrations of a stretched wire; for here the forces brought into play, as explained in Lecture IV., are different. But the longitudinal vibrations of a stretched wire do follow the same order as the longitudinal vibrations of a rod fixed at both ends, for here the forces brought into play are the same, being in both cases the elasticity of the material.

A rod fixed at one end vibrates longitudinally as a whole, or it divides into two, three, four, &c., vibrating parts, separated from each other by nodes. The order of the tones of such a rod is that of the odd numbers 1, 3, 5, 7, &c.

A rod free at both ends can also vibrate longitudinally. Its lowest note corresponds to a division of the rod into two vibrating parts by a node at its centre. The overtones of such a rod correspond to its division into three, four, five, &c., vibrating parts, separated from each other by two, three, four, &c., nodes. The order of the tones of such a rod is that of the numbers 1, 2, 3, 4, 5, &c.

We may also express the order by saying that while

the tones of a rod free at one end follow the order of the odd numbers 1, 3, 5, 7, &c., the tones of a rod free at both ends follows the order of the even numbers 2, 4, 6, 8, &c.

At the points of maximum vibration the rod suffers no change of density; at the nodes, on the contrary, the changes of density reach a maximum. This may be proved by the action of the rod upon polarised light.

Columns of air of definite length resound to tuning-forks of definite rates of vibration.

The length of a tube filled with air, and closed at one end, which resounds to a fork, is one-fourth of the length of the sonorous wave produced by the fork.

This resonance is due to the synchronism which exists between the vibrating period of the fork and that of the column of air.

By blowing across the mouth of a tube closed at one end, we produce a flutter of the air, and some pulse of this flutter may be raised by the resonance of the tube to a musical sound.

The sound is the same as that obtained when a tuning-fork, whose rate of vibration is that of the tube, is placed over the mouth of the tube.

When a tube closed at one end—a stopped organ-pipe for example—sounds its lowest note, the column of air within it is undivided by a node. The overtones of such a column correspond to its division into parts like those of a rod fixed at one end and vibrating longitudinally. The order of its tones is that of the odd numbers 1, 3, 5, 7, &c. That this must be the order follows from the manner in which the column is divided.

In organ-pipes the air is agitated by causing it to issue from a narrow slit, and to strike upon a cutting edge. Some pulse of the flutter thus produced is raised by the resonance of the pipe to a musical sound.

When, instead of the aerial flutter, a tuning-fork of the proper rate of vibration is placed at the embouchure of an organ-pipe, the pipe *speaks* in response to the fork. In practice, the organ-pipe virtually creates its own tuning-fork, by compelling the sheet of air at its embouchure to vibrate in periods synchronous with its own.

An open organ-pipe yields a note an octave higher than that of a closed pipe of the same length. This relation is a necessary consequence of the respective modes of vibration.

When, for example, a stopped organ-pipe sounds its deepest note, the column of air, as already explained, is undivided. When an open pipe sounds its deepest note, the column is divided by a node at its centre. The open pipe in this case virtually consists of two stopped pipes with a common base. Hence it is plain that the fundamental note of an open pipe must be the same as that of a stopped pipe of half its length.

The length of a stopped pipe is one fourth that of the sonorous wave which it produces, while the length of an open pipe is one-half that of its sonorous wave.

The order of the tones of an open pipe is that of the even numbers 2, 4, 6, 8, &c., or of the natural numbers 1, 2, 3, 4, &c.

In both stopped and open pipes the number of vibrations executed in a given time is inversely proportional to the length of the pipe.

The places of maximum vibration in organ-pipes are places of minimum changes of density; while at the places of minimum vibration the changes of density reach a maximum.

The velocities of sound in gases, liquids, and solids may be inferred from the tones which equal lengths of them produce; or they may be inferred from the lengths of these substances which yield equal tones.

Reeds, or vibrating tongues, are often associated with vibrating columns of air. They consist of flexible laminæ which vibrate to and fro in a rectangular orifice, thus rendering intermittent the air-current passing through the orifice.

The action of the reed is the same as that of the syren.

Flexible wooden reeds are sometimes compelled to vibrate in unison with the column of air in the associated pipe; other reeds are too stiff to be thus controlled by the vibrating air. In this latter case the column of air is taken of such a length that its vibrations synchronise with those of the reed.

By associating suitable pipes with reeds we impart to their tones the qualities of the human voice.

The vocal organ in man is a reed instrument, the vibrating reed in this case being elastic bands placed at the top of the trachea, and capable of various degrees of tension.

The rate of vibration of these vocal chords is practically uninfluenced by the resonance of the mouth; but the mouth by changing its shape, can be caused to resound to the fundamental tone, or to any of the overtones of the vocal chords.

By the strengthening of particular tones through the resonance of the mouth, the clang-tint of the voice is altered.

The different vowel sounds are produced by different admixtures of the fundamental tone and the overtones of the vocal chords.

Bell's telephone depends on the capacity of a thin iron plate to take up the vibrations of the human voice; on the capacity of a magnet to respond by slight changes of its magnetism to the vibrations of such a plate; on the capacity of such changes to evoke electric currents corresponding to them in strength, direction, and duration;

and, finally, on the capacity of these currents to be transmitted to a distance, and there to reproduce vibrations exactly similar to those which gave the currents birth.

In Edison's telephone the first agent employed is an ordinary voltaic current which traverses the entire circuit. At a certain point in the circuit a thin cake of fine lamp-black is gently pressed between two thin plates of platinum, the current passing across the plates. On one of these plates the voice impinges, and its vibrations, producing variations in the closeness of the contact of platinum and carbon, produce thereby corresponding variations in the strength of the voltaic current.

At another point of the circuit is introduced a small induction coil, through the primary wire of which the voltaic current passes. Every variation in the strength of the voltaic current produces an induced current in the secondary wire of the small induction coil. The induced currents pass from a metal rubber to a cylinder formed of a mixture of powdered chalk and certain mineral salts, pressed to compactness in a mould and moistened by water. This cylinder is kept by the hand in a state of rotation. Variations in the bite of the metal rubber are produced by the electrolytic action of the induced currents. The cylinder is thus caused to move by jerks corresponding to the vibrations of the voice, and these jerks communicated to a thin plate of mica connected with the rubber reproduce the voice that generated them.

In the microphone a voltaic current is caused to pass between two bits of carbon pressing lightly against each other. A Bell's telephone is also introduced into the circuit. The slightest shaking of the bits of carbon, a shaking which might be produced by the tread of an insect, causes variations in the contact between the bits of carbon, and corresponding variations in the strength of the current passing round the telephone. These

variations, by affecting the telephone magnet, enable it to throw its ferrotype plate into vibration, thus making, as it were, the tread of the insect audible. It must be remembered, however, that the part played by the insect is simply the mechanical one of altering the contact of the carbons.

In the phonograph the vibrations of the voice are conveyed from a thin ferrotype plate, on which the voice impinges, to a point which presses gently on a sheet of tinfoil wrapped round a cylinder. When the cylinder rotates, indentations are produced upon the surface of the foil. Over these indentations the point which produced them, or better still a second point, is caused to pass. By the tapping of the point against the foil, the voice which produced the indentations is amusingly but imperfectly imitated.

When the solid substance of a tube stopped at one, or at both ends, is caused to vibrate longitudinally, the air within it is also thrown into vibration.

By covering the interior surface of the tube with a light powder, the manner in which the aerial column divides itself may be rendered apparent. From the division of the column the velocity of sound in the substance of the tube compared with its velocity in air, may be inferred.

Other gases may be employed instead of air, and the velocity of sound in these gases compared with its velocity in the substance of the tube, may be determined.

The end of a rod vibrating longitudinally may be caused to agitate a column of air contained in a tube compelling the air to divide itself into ventral segments. These segments may be rendered visible by light powders, and from them the velocity of sound in the substance of the vibrating rod, compared with its velocity in air, may be inferred.

In this way the relative velocities of sound in all solid substances capable of being formed into rods, and of vibrating longitudinally, may be determined.

Gases and vapours are diathermanous in different degrees. A perfectly diathermanous body would absorb no radiant heat, and only those gases and vapours which absorb it suffer change of temperature. When an absorbing gas or vapour is exposed to an intermittent beam, during the moments of exposure expansion occurs, followed by contraction when the beam is intercepted. This rhythmic action produces sonorous pulses which, when sufficiently rapid, unite to musical sounds.

The intensity of the sound depends on the magnitude of the absorption of radiant heat; varying from a maximum in chloride of methyl where the absorption is greatest, to a minimum in dry air where the absorption is sensibly *nil.*

LECTURE VI.

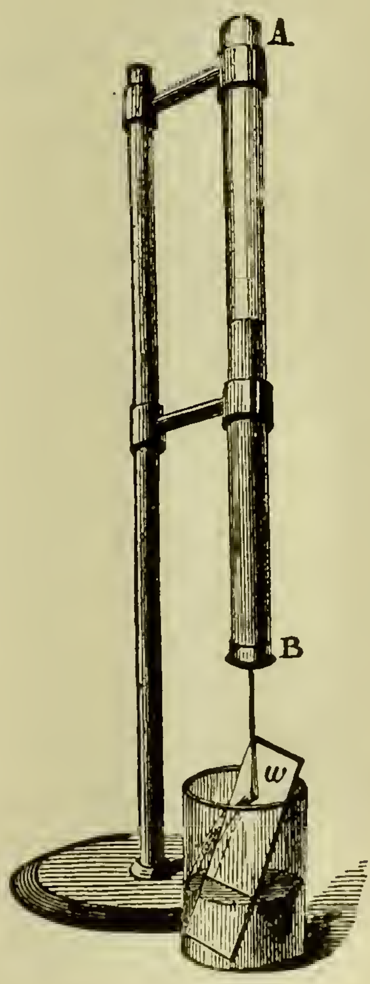
SINGING FLAMES—INFLUENCE OF THE TUBE SURROUNDING THE FLAME—INFLUENCE OF SIZE OF FLAME—HARMONIC NOTES OF FLAMES—EFFECT OF UNISONANT NOTES ON SINGING FLAMES—ACTION OF SOUND ON NAKED FLAMES—EXPERIMENTS WITH FISH-TAIL AND BAT'S-WING BURNERS—EXPERIMENTS ON TALL FLAMES—EXTRAORDINARY DELICACY OF FLAMES AS ACOUSTIC REAGENTS—THE VOWEL FLAME—ACTION OF CONVERSATIONAL TONES UPON FLAMES—ACTION OF MUSICAL SOUNDS ON SMOKE JETS—CONSTITUTION OF WATER JETS—PLATEAU'S THEORY OF THE RESOLUTION OF A LIQUID VEIN INTO DROPS—ACTION OF MUSICAL SOUNDS ON WATER JETS—A LIQUID VEIN MAY COMPETE IN POINT OF DELICACY WITH THE EAR.

§ 1. *Rhythm of Friction: Musical Flow of a Liquid through a Small Aperture.*

FRICTION is always rhythmic. When a resined bow is passed across a string, the tension of the string secures the perfect rhythm of the friction. When the wetted finger is moved round the edge of a glass, the breaking up of the friction into rhythmic pulses expresses itself in music. Savart's beautiful experiments on the flow of liquids through small orifices bear immediately upon this question. We have here the means of verifying his results. The tube A B, fig. 118, is filled with water, its extremity, B being closed by a plate of brass, which is pierced by a circular orifice of a diameter equal to the thickness of the plate. Removing a little peg which stops the orifice, the water issues from it, and as it sinks in the tube a musical note of great sweetness issues from the liquid column. This note is due to the intermittent flow of the liquid through the orifice, by which the whole column above it is thrown into vibration. The tendency to this

effect shows itself when tea is poured from a teapot, in the circular ripples that cover the falling liquid. The same intermittence is observed in the black dense smoke which rolls in rhythmic rings from the funnel of a steamer. The unpleasant noise of unoiled machinery is also a declaration of the fact that the friction is not uniform, but is due to the alternate 'bite' and release of the rubbing surfaces.

Fig. 118.



Where gases are concerned friction is of the same intermittent character. A rifle bullet sings in its passage through the air; while to the rubbing of the wind against the boles and branches of the trees are to be ascribed the 'waterfall tones' of an agitated pine-wood. Pass a steadily burning candle rapidly through the air; an indented band of light, declaring intermittence, is often the consequence, while the almost musical sound which accompanies the appearance of this band is the audible expression of the rhythm. On the other hand, if you blow gently against a candle flame, the fluttering noise announces a rhythmic action. We have already learned what can be done when a pipe is associated with such a flutter; we have learned that the pipe selects a special pulse from the flutter, and raises it by resonance to a musical sound. In a similar manner the noise of a flame may be turned to account. The blowpipe flame of our laboratory, for example, when enclosed within an appropriate tube, has its flutter raised to a *roar*. The special pulse first selected soon reacts upon the flame so as to abolish in a great degree the other pulses, com-

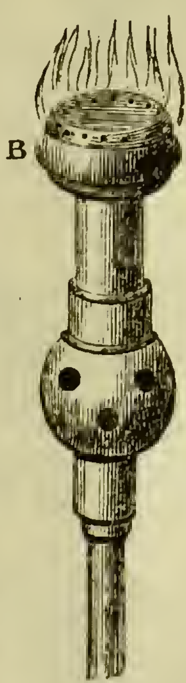
pelling the flame to vibrate in periods answering to the selected one. And this reaction can become so powerful—the timed shock of the reflected pulses may accumulate to such an extent—as to beat the flame, even when very large, into extinction.

§ 2. *Musical Flames.*

Nor is it necessary to produce this flutter by any extraneous means. When a gas flame is simply enclosed within a tube the passage of the air over it is usually sufficient to produce the necessary rhythmic action, so as to cause the flame to burst spontaneously into song. This flame-music may be rendered exceedingly intense. Over a flame issuing from a ring burner with twenty-eight orifices, I place a tin tube, 5 feet long, and $2\frac{1}{2}$ inches in diameter. The flame flutters at first, but it soon chastens its impulses into perfect periodicity, and a deep and clear musical tone is the result. By lowering the gas the note now sounded is caused to cease, but after a momentary interval of silence, another note, which is the octave of the last, is yielded by the flame. The first note was the fundamental note of the surrounding tube: this second note is its first harmonic. Here, as in the case of open organ-pipes, we have the aerial column dividing itself into vibrating segments, separated from each other by nodes.

A still more striking effect is obtained with this larger tube, *a b*, fig. 119, 15 feet long, and 4 inches wide, which was made for a totally different purpose. It is supported by a steady stand *s s′*, and into it is lifted the tall burner shown enlarged at B. You hear the incipient flutter, you now hear the more powerful sound. As the flame is lifted higher the action becomes more violent, until finally a storm of music issues from the tube. And now all has suddenly ceased; the reaction of its own pulses upon

the flame has quenched it. I relight the flame and make it very small. When raised within the tube, it sings, but it is one of the harmonics of the tube that you now hear. On turning the gas fully on, the note ceases—all is silent for a moment; but the storm is brewing and soon it bursts forth, as at first, in a kind of hurricane of sound. By lowering the flame the fundamental note is abolished, and now you hear the first harmonic of the tube. Making the flame still smaller, the first harmonic disappears, and the second is heard. Your ears being disciplined to the apprehension of these sounds, I turn the gas once more fully on. Mingling with the deepest note you notice the harmonics, as if struggling to be heard amid the general uproar of the flame. With a large Bunsen's rose burner, the sound of this tube becomes powerful enough to shake the floor and seats, and the large audience that occupies the seats of this room, while the extinction of the flame, by the reaction of the sonorous pulses, announces itself by an explosion almost as loud as a pistol shot. It must occur to you that a chimney is a tube of this kind upon a large scale,



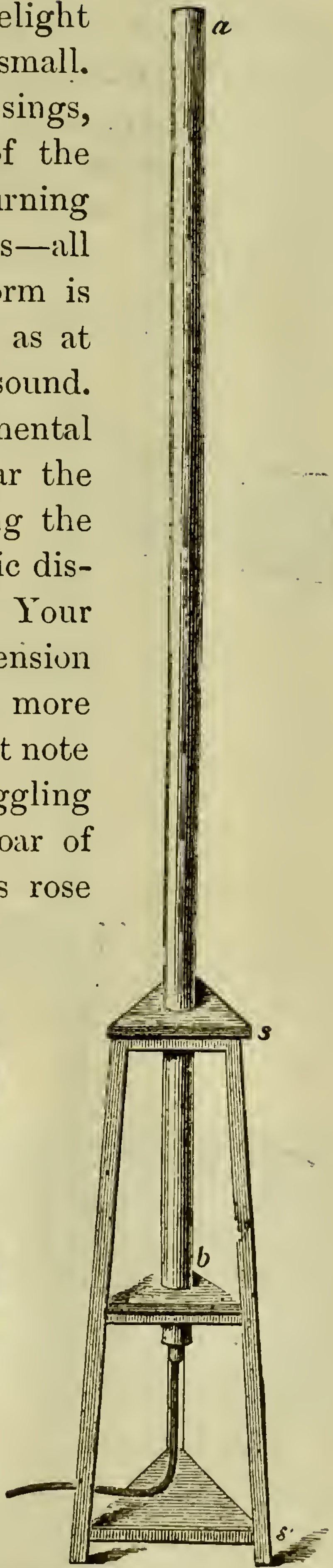
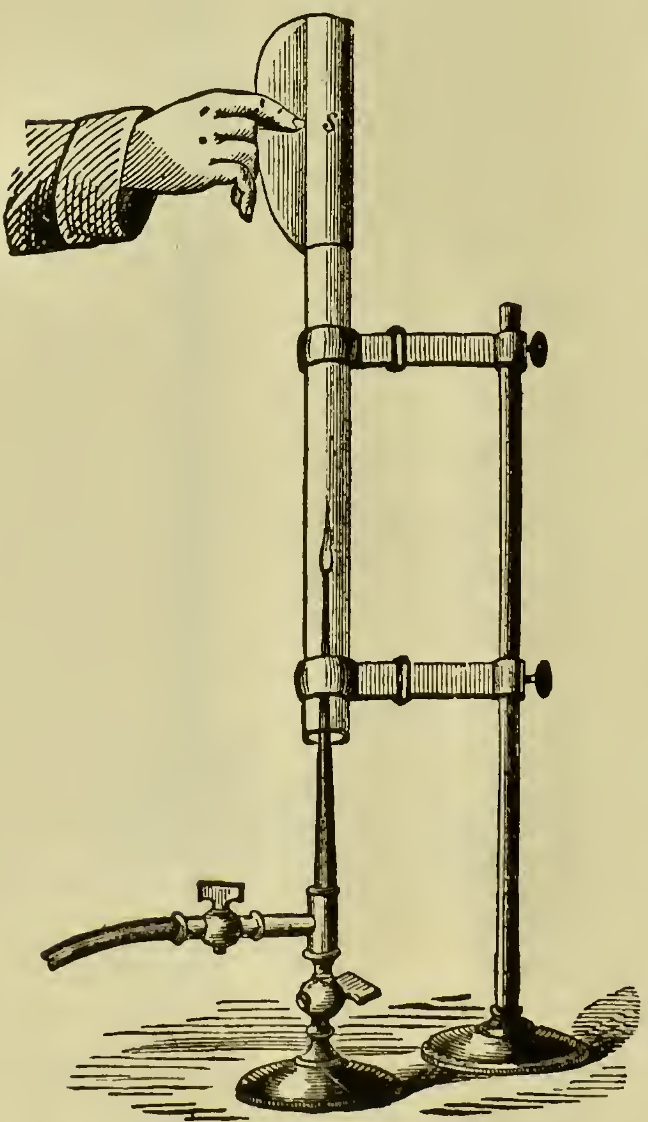


Fig. 119.

and that the roar of a flame in a chimney is simply a rough attempt at music.

Let us now pass on to shorter tubes and smaller flames. Placing tubes of different lengths over eight small flames, each of them starts into song, and you notice that as the tubes lengthen the tones deepen. The lengths of these tubes are so chosen that they yield in succession the eight notes of the gamut. Round some of them you observe a paper slider, s, fig. 120, by which the tube can be lengthened or shortened. If while the flame is sounding the slider be raised, the pitch instantly falls; if lowered the pitch rises. These experiments prove the flame to be governed by the tube. By the reaction of the pulses, reflected back upon the flame, its flutter is rendered perfectly periodic, the length of that period being determined, as in the case of organ pipes, by the length of the tube.

Fig. 120.



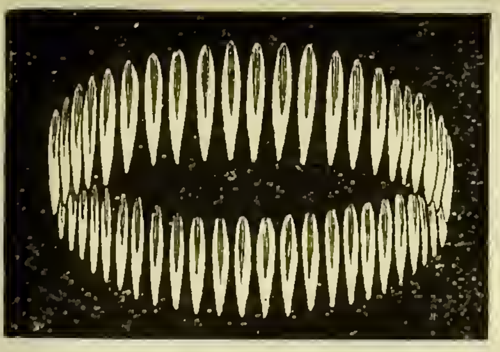
The fixed stars, especially those near the horizon, shine with an unsteady light, sometimes changing colour as they twinkle. I have often watched at night, upon the plateaux of the Alps, the alternate flash of ruby and emerald in the lower

and larger stars. If you place a piece of looking-glass so that you can see in it the image of such a star; on tilting the glass quickly to and fro, the line of light obtained will not be continuous, but will form a string of coloured beads of extreme beauty. The same effect is obtained when an opera-glass is pointed to the star and shaken. This experiment shows that in the act of twinkling the light of the star is quenched at intervals; the dark spaces between the bright beads corresponding to the periods of extinction. Now, our singing flame is a *twinkling* flame. When it begins to sing you observe a certain quivering motion which may be analysed as in the case of a star.[1] I can now see the image of this flame in a small looking-glass. On continually tilting the glass, so as to cause the image to form a circle of light, the luminous band is not seen to be continuous, as it would be if the flame were perfectly steady; it is resolved into a beautiful chain of flames, fig. 121.

§ 3. *Experimental Analysis of Musical Flame.*

With a larger, brighter, and less rapidly vibrating flame, you may all see this intermittent action. Over this gas flame *f*, fig. 122, is placed a glass tube A B, 6 feet long, and 2 inches in diameter. The back of the tube is blackened, so as to prevent the light of the flame from falling directly upon the screen, which it is now desirable to have as dark as possible. In front of the tube is placed a concave mirror, M, which forms upon the screen an enlarged image of the flame. I turn the mirror with

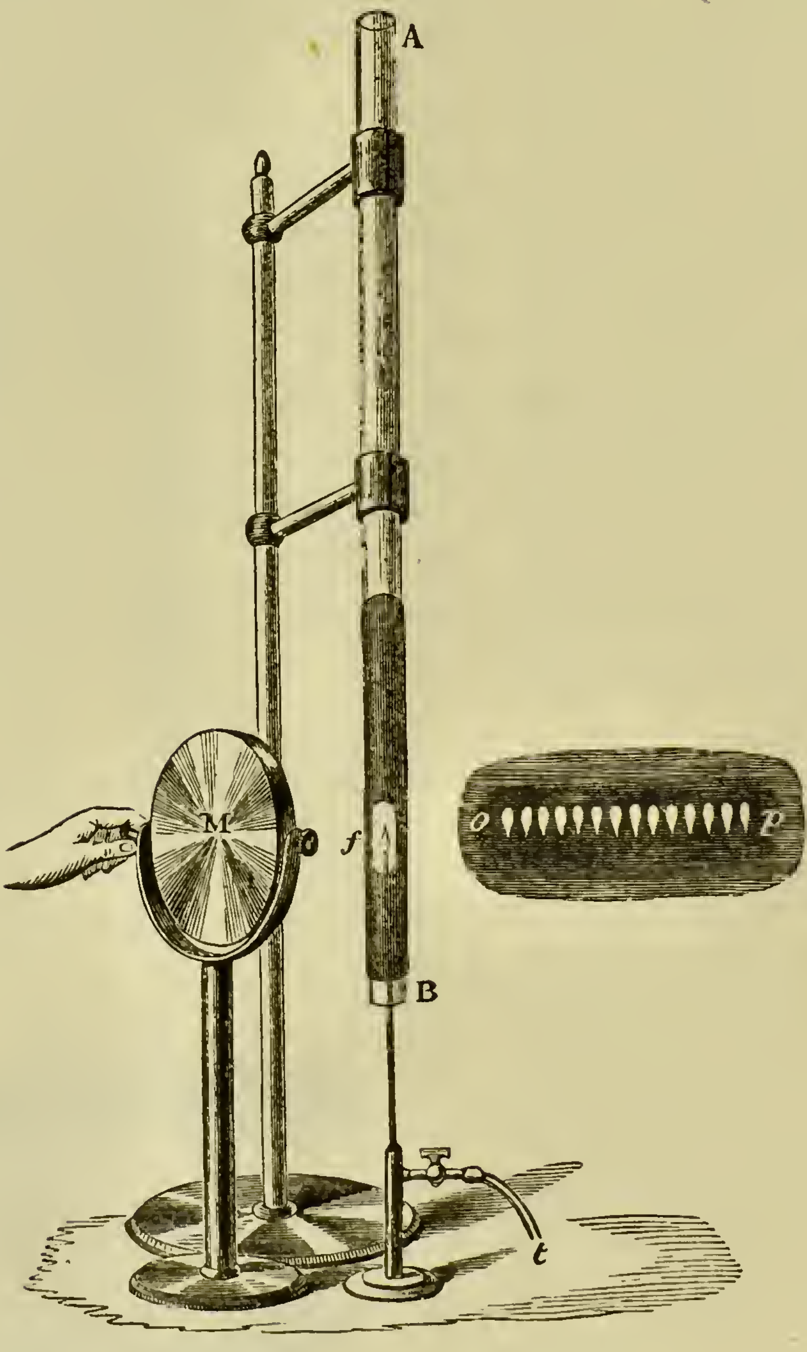
FIG. 121.



[1] This experiment was first made with a hydrogen flame by Sir C. Wheatstone.

my hand and cause the image to pass over the screen Were the flame silent and steady, we should obtain a *continuous* band of light; but it quivers, and emits at the same time a deep note. On twirling the mirror,

Fig. 122.



therefore, we obtain, instead of a continuous band, a luminous chain of images. By fast turning, these images are drawn more widely apart; by slow turning, they are caused to close up, the chain of flames passing through the most beautiful variations. Clasping the

lower end, B, of the tube with my hand, I so impede the air as to stop the flames' vibration; a continuous band is the consequence. Observe the suddenness with which this breaks up into a rippling line of images the moment my hand is removed, and the current of air permitted to pass over the flame.

Rijke's Sounding Flame.

Rijke employed an ingenious and a previously unknown mode of throwing a column of air into musical vibration. Within a tube, and transversely across it, he placed a diaphragm of wire gauze, which he heated to redness either by a flame or by a voltaic current. In the former case the sound, though it may be at first very forcible, soon ceases. In the latter case the sound is permanent, because of the permanent heating of the wire. The tubes employed may be of glass or metal, and of various dimensions. When very large, and when the gauze is highly heated, the sounds produced are very loud.

Rijke found the position of the wire gauze corresponding to the maximum effect to be one fourth of the length of the tube from its lower end. The gauze is rapidly brought to incandescence by a Bunsen flame The flame is then withdrawn, and a note, dependent, as regards pitch, on the length of the tube and the temperature of the aerial column, is immediately produced. The sound sinks as the temperature of the gauze falls, and ceases when gauze and air are at the same temperature. The sound in Rijke's tubes is obviously due to the sudden dilatation of the air in passing through the hot gauze. The first dilatation produces a pulse which, behaving in the usual way in the open tube, renders the abstraction of heat from the gauze periodic. An experiment in some sense complementary to that of Rijke was made by Bosschia

and Reiss, who produced musical sounds by causing hot air to impinge on cold gauze.

§ 4. *Rate of Vibration of Flame: Toepler's Experiment.*

When a *small* vibrating coal-gas flame is carefully examined by the rotating mirror, the beaded line is a series of yellow-tipped flames, each resting upon a base of the richest blue. In some cases I have been unable to observe any union of one flame with another: the spaces between the flames being absolutely dark to the eye. But, if dark, the flame must have been totally extinguished at intervals, a residue of heat, however, remaining sufficient to reignite the gas. This is at least possible, for gas may be ignited by non-luminous air.[1] By means of the syren, we can readily determine the number of times this flame extinguishes and relights itself in a second. As the note of the instrument approaches that of the flame, unison is preceded by the well-known beats, which become gradually less rapid, until the two notes melt finally into perfect unison. Maintaining the syren at this pitch for a minute, at the end of that time we have recorded upon our dials 1,700 revolutions. But the disc being perforated by 16 holes, it follows that every revolution corresponds to 16 pulses. Multiplying 1,700 by 16, we find the number of pulses in a minute to be 27,200. This number of times did the flame extinguish and rekindle itself during the continuance of the experiment, that is to say, it was put out and relighted 453 times in a second.

A flash of light, though instantaneous, makes an impression upon the retina which endures for the tenth of a second or more. A flying rifle-bullet, illuminated by a single flash of lightning, would seem to stand still in the air for the tenth of a second. A black disk with radial

[1] A gas-jet, for example, can be ignited five inches above the tip of a visible gas flame, where platinum leaf shews no redness.

white strips, when moderately illuminated and rapidly rotated, causes the white and black to blend to grey; while a spark of electricity, or a flash of lightning, reduces the disk to apparent stillness, the white radial strips being for a time plainly seen. Now, the singing flame is a flashing flame, and M. Toepler has shown that by causing a striped disk to rotate at the proper speed in the presence of such a flame the disk is brought to apparent stillness, the white stripes being rendered plainly visible. The experiment is both easy and interesting.

§ 5. *Harmonic Sounds of Flame.*

A singing flame yields so freely to the pulses falling upon it that it is almost wholly governed by the surrounding tube: *almost*, but not altogether. The pitch of the note depends in some measure upon the size of the flame. This is readily proved, by causing two flames to emit the same note, and then slightly altering the size of either of them. The unison is instantly disturbed by beats. By altering the size of a flame we can also, as already illustrated, draw forth the harmonic overtones of the tube which surrounds it. This experiment is best performed with hydrogen, its combustion being much more vigorous than that of ordinary gas. When a glass tube 7 feet long is placed over a large hydrogen flame, the fundamental note of the tube is obtained, corresponding to a division of the column of air by a single node at the centre. Placing a second tube, 3 feet 6 inches long, over the same flame, no musical sound whatever is obtained; the large flame, in fact, is not able to accommodate itself to the vibrating period of the shorter tube. But, on lessening the flame, it soon bursts into vigorous song, its note being the octave of that yielded by the longer tube. Removing the shorter tube, I once more cover the flame with the longer one. It no longer sounds its

fundamental note, but the precise note of the shorter tube. To accommodate itself to the vibrating period of the diminished flame, the longer column of air divides itself like an open organ-pipe when it yields its first harmonic. By varying the size of the flame, it is possible, with the tube now before you, to obtain a series of notes whose rates of vibration are in the ratio of the numbers 1 : 2 : 3 : 4 : 5, that is to say, the fundamental tone and its first four harmonics.

These sounding flames, though probably never before raised to the intensity, or shown in the variety, in which they have been exhibited here to-day, are of old standing. In 1777, the sounds of a hydrogen flame were heard by Dr. Higgins. In 1802, they were investigated to some extent by Chladni, who also refers to an incorrect account of them given by De Luc. Chladni showed that the tones are those of the open tube which surrounds the flame, and he succeeded in obtaining the two first harmonics. In 1802, G. De la Rive experimented on this subject. Placing a little water in the bulb of a thermometer, and heating it, he showed that musical tones of force and sweetness could be produced by the periodic condensation of the vapour in the stem of the thermometer. He accordingly referred the sounds of hydrogen flames to the alternate expansion and condensation of the aqueous vapour produced by the combustion. We can readily imitate his experiments. Holding, with its stem oblique, a thermometer bulb containing water in the flame of a spirit lamp, the sounds are heard, soon after the water begins to boil. In 1818, however, Faraday showed that the tones are produced when the tube surrounding the flame is placed in air of a temperature higher than 100° C., condensation being then impossible. He also showed that the tones could be obtained from flames of carbonic oxide, where aqueous vapour is entirely out of the question.

§ 6. *Action of Extraneous Sounds on Flame: Experiments of Schaffgotsch and Tyndall.*

After these experiments, the first novel acoustic observation on flames was made in Berlin by the late Count Schaffgotsch, who showed that when an ordinary gas flame was surmounted by a short tube, a strong falsetto voice pitched to the note of the tube, or to its higher octave, caused the flame to quiver. In some cases, when the note of the tube was high, the flame could even be extinguished by the voice.

In the spring of 1857, this experiment came to my notice. No directions were given in the short account of it published in Poggendorff's 'Annalen'; but, in endeavouring to ascertain the conditions of success, a number of singular effects forced themselves upon my attention. Meanwhile, Count Schaffgotsch also followed up the subject. To a great extent we travelled over the same ground, neither of us knowing how the other was engaged; but so far as the experiments then executed are common to us both, to Count Schaffgotsch belongs the priority.

Let me here repeat his first observation. Within a glass tube, 11 inches long, burns a small gas flame, bright and silent. The note of the tube has been ascertained; and now, standing at some distance from the flame, I sound that note; the flame *quivers*. To produce *extinction* it is necessary to employ a burner with a very narrow aperture, from which the gas issues under considerable pressure. On gently singing the note of the tube surrounding such a flame, it quivers; but on throwing more power into the voice the flame is extinguished

The cause of the quivering of the flame will be best revealed by an experiment with the syren. As the note of the syren approaches that of the flame you hear beats, and at the same time you observe a dancing of the flame syn-

chronous with the beats. The jumps succeed each other more slowly as unison is approached. They cease when the unison is perfect, and they begin again as soon as the syren is urged beyond unison, becoming more rapid as the discordance is increased. The cause of the quiver observed by M. Schaffgotsch was revealed to me by this experiment. The flame jumped because the note of the tube surrounding it was nearly, but not quite, in unison with the voice of the experimenter.

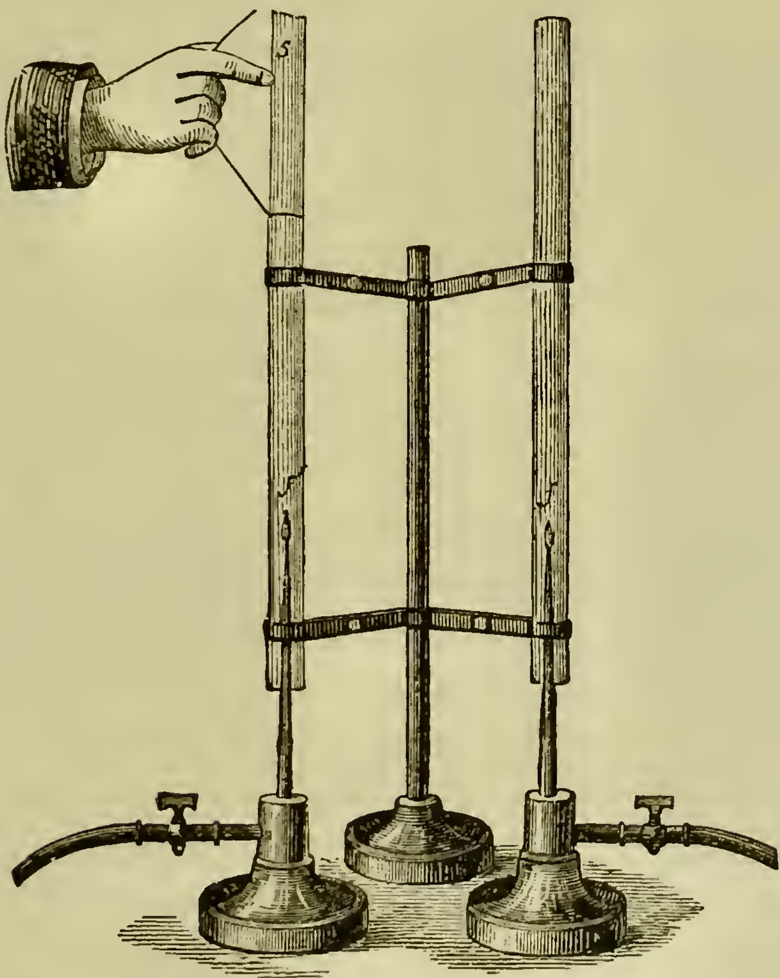
That the jumping of the flame proceeds in exact accordance with the beats is well shown by a tuning-fork which yields the same note as the flame. Loading such a fork with a bit of wax, so as to throw it slightly out of unison, and bringing it, when agitated, near the tube in which the flame is singing, the beats and the leaps of the flame occur at the same intervals. When the fork is placed over a resonant jar, all of you can hear the beats, and see at the same time the dancing of the flame. By changing the load upon the tuning-fork, or by slightly altering the size of the flame, the rate at which the beats succeed each other may be altered; but in all cases the jumps address the eye precisely when the beats address the ear.

During these experiments I noticed that, on raising the voice to the proper pitch, a silent flame could be caused to sing. The same observation had, without my knowledge, been made a short time previously by Count Schaffgotsch. A tube, 12 inches long, is placed over a flame which stands about an inch and a half above the lower end of the tube. When the proper note is sounded the flame trembles, but it does not sing. When the tube is lowered until the flame is three inches from its end, the song is spontaneous. Between these two positions there is a third, at which, if the flame be placed, it will burn silently; but if it be excited by the voice it will sing, and continue to sing.

When a silent flame, capable of being excited in the manner here described, is looked at in a moving mirror, it produces there a continuous band of light. Nothing can be more beautiful than the sudden breaking up of this band into a string of richly luminous pearls at the instant the voice is pitched to the proper note.

One singing flame may be caused to effect the musical ignition of another. Before you are two small flames, f'

Fig. 123.



and f, fig. 123, the tube over f' being $10\frac{1}{2}$ inches, and that over f 12 inches long. The shorter tube is clasped by a paper slider s. The flame f' is now singing, but the flame f, in the longer tube, is silent. I raise the paper slider which surrounds f', so as to lengthen the tube, and on approaching the pitch of the tube surrounding f, that flame sings. The experiment may be varied by

making f the singing flame, and f' the silent one at starting. Raising the telescopic slider, a point is soon attained where the flame f' commences its song. In this way one flame may excite another through considerable distances. It is also possible to silence the singing flame by the proper management of the voice.

SENSITIVE NAKED FLAMES.

§ 7. *Discovery of Sensitive Flames by Leconte.*

We have hitherto dealt with flames surrounded by resonant tubes; and none of these flames, if naked, would respond in any way to such noise or music as could be here applied. Still it is possible to make naked flames thus sympathetic. This action of musical sounds upon naked flames was first observed by Professor Leconte at a musical party in the United States. His observation is thus described:—'Soon after the music commenced, I observed that the flame exhibited pulsations which were *exactly synchronous* with the audible beats. This phenomenon was very striking to every one in the room, and especially so when the strong notes of the violoncello came in. It was exceedingly interesting to observe how perfectly even the *trills* of this instrument were reflected on the sheet of flame. *A deaf man might have seen the harmony.* As the evening advanced, and the diminished consumption of gas in the city *increased the pressure*, the phenomenon became more conspicuous. The *jumping* of the flame gradually increased, became somewhat irregular, and, finally, it began to flare continuously, emitting the characteristic sound indicating the escape of a greater amount of gas than could be properly consumed. I then ascertained by experiment, that the phenomenon *did not* take

place unless the discharge of gas was so regulated that the flame approximated to the condition of *flaring*. I likewise determined by experiment, that the effects *were not* produced by jarring or shaking the floor and walls of the room by means of repeated concussions. Hence it is obvious that the pulsations of the flame *were not* owing to *indirect* vibrations propagated through the medium of the walls of the room to the burning apparatus, but must have been produced by the *direct* influence of aerial sonorous pulses on the burning jet.'[1]

The significant remark, that the jumping of the flame was not observed until it was near flaring, suggests the

FIG. 124.

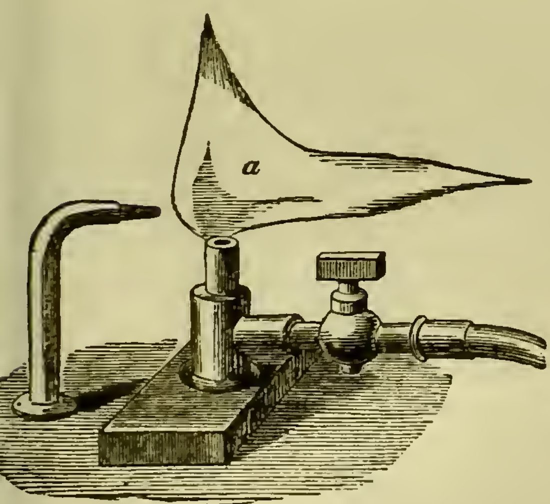
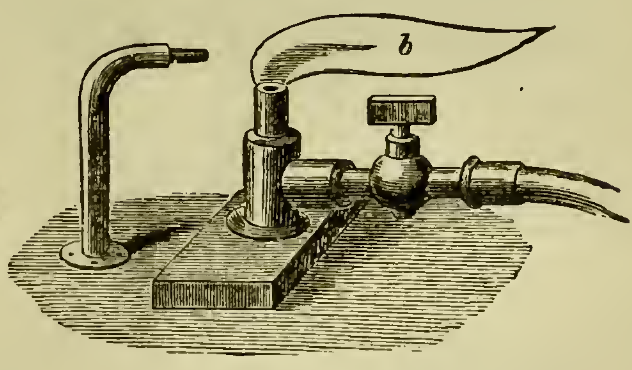


FIG. 125



means of repeating the experiments of Professor Leconte; while a more intimate knowledge of the conditions of success enables us to vary and exalt the effect in an extraordinary degree. Before you burns a bright candle-flame, but no sound that can be produced here has any effect upon it. Though sonorous waves of great power be sent through the air the candle-flame remains insensible.

But by proper precautions even a candle-flame may be rendered sensitive. Urging from a small blow-pipe a narrow stream of air through such a flame, an incipient

[1] 'Philosophical Magazine,' March 1858, p. 235. In the Appendix Professor Leconte's interesting paper is given *in extenso*.

flutter is produced. The flame then jumps visibly to the sound of a whistle, or to a chirrup. The experiment may be so arranged that when the whistle sounds, the flame shall be either restored almost to its pristine brightness or that the small amount of light it still possesses shall disappear.

The blow-pipe flame of our laboratory is totally unaffected by the sound of the whistle as long as no air is urged through it. By properly tempering the force of the blast, a flame is obtained of the shape shown in fig. 124 On sounding the whistle the erect portion of the flame drops down, and while the sound continues the flame maintains the form shown in fig. 125.

§ 8. *Experiments on Fish-tail and Bat's-wing Flames.*

We now pass on to a thin sheet of flame, issuing from a common fish-tail burner, fig. 126. You might sing to

Fig. 126.

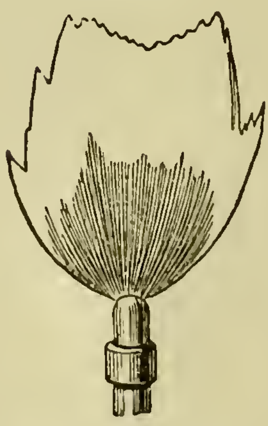


Fig. 127.

this flame, varying the pitch of your voice, but no shiver of the flame would be visible. You might employ pitch-pipes, tuning-forks, bells, and trumpets, with a like ab-

sence of all effect. A barely perceptible motion of the interior of the flame may be noticed when a shrill whistle is blown close to it. But by turning the cock more fully on, the flame is brought to the verge of flaring. And now, when the whistle is blown, the flame thrusts suddenly out seven quivering tongues, fig. 127. The moment the sound ceases, the tongues disappear, and the flame becomes quiescent.

Passing from a fish-tail to a bat's-wing burner, we obtain a broad, steady flame, fig. 128. It is quite insens-

Fig. 128.

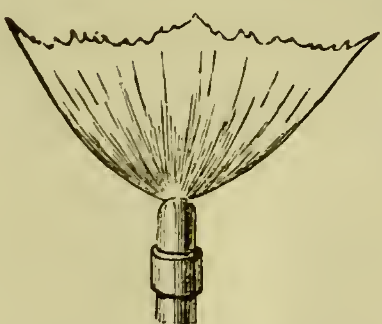


Fig. 129.

ible to the loudest sound which would be tolerable here. The flame is fed from a small gas-holder.[1] Increasing gradually the pressure, a slight flutter of the edge of the flame at length answers to the sound of the whistle. Turning on the gas until the flame is on the point of roaring, and blowing the whistle, it roars, and suddenly assumes the form shown in fig. 129.

When a distant anvil is struck with a hammer, the flame instantly responds by thrusting forth its tongues.

An essential condition to entire success in these experiments disclosed itself in the following manner. I was

[1] A gas-bag properly weighted also answers for these experiments.

operating on two fish-tail flames, one of which jumped to a whistle while the other did not. The gas of the non-sensitive flame was turned off, additional pressure being thereby thrown upon the other flame. It flared, and its cock was turned so as to lower the flame; but it now proved non-sensitive, however close it might be brought to the point of flaring. The narrow orifice of the half-turned cock interfered with the action of the sound. When the cock was turned fully on, the flame being lowered by opening the cock of the other burner, it became again sensitive. Up to this time a great number of burners had been tried, but with many of them, the action was *nil*. Acting, however, upon the hint conveyed by this observation, I had the cocks which fed the flames more widely opened. Our most refractory burners were thus rendered sensitive.

In this way the observation of Prof. Leconte is easily and strikingly illustrated; in our subsequent, and far more delicate experiments, the precaution just referred to is still more essential.

§ 9. *Experiments on Flames from Circular Apertures.*

A long flame may be shortened and a short one lengthened, according to circumstances, by sonorous vibrations. The flame shown in fig. 130 is long, straight, and smoky; that in fig. 131 is short, forked, and brilliant. On sounding the whistle, the long flame becomes short, forked, and brilliant, as in fig. 132; while the forked flame becomes long and smoky, as in fig. 133. As regards, therefore, their response to the sound of the whistle, one of these flames is the complement of the other.

In fig. 134 is represented another smoky flame which, when the whistle sounds, breaks up into the form shown in fig. 135.

When a brilliant sensitive flame illuminates an otherwise dark room, in which a suitable bell is caused to strike, a series of periodic quenchings of the light by the sound occurs. Every stroke of the bell is accompanied by a momentary darkening of the room.

The foregoing experiments illustrate the lengthening and shortening of flames by sonorous vibrations. They may also produce *rotation*. From some of our home-

Fig. 130.

Fig. 131.

Fig. 132.

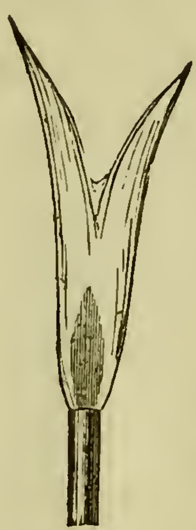


Fig. 133.

Fig. 134.

Fig. 135.

made burners issue flat flames, about ten inches high, and three inches across at their widest part. When the whistle sounds, the plane of each flame turns ninety degrees round, and continues in its new position as long as the sound continues.

A flame of admirable steadiness and brilliancy now burns before you. It issues from a circular orifice in a common iron nipple. This burner, which requires

great pressure to make its flame flare, has been specially chosen for the purpose of enabling you to observe, with distinctness, the gradual change from apathy to sensitiveness. The flame, now 4 inches high, is quite indifferent to sound. On increasing the pressure its height becomes 6 inches; but it is still indifferent. When its length is 12 inches, a barely perceptible quiver responds to the whistle. When 16 or 17 inches high, it jumps briskly the moment the anvil is tapped or the whistle sounded. When the flame is 20 inches long you observe a quivering at intervals, which announces that it is near roaring. A slight increase of pressure causes it to roar, and shorten at the same time to 8 inches.

Diminishing the pressure a little, the flame is again 20 inches long, but it is on the point of roaring and shortening. Like the singing flames which were started by the voice, it stands on the brink of a precipice. The proper note pushes it over. It shortens when the whistle sounds, exactly as it did when the pressure is in excess. The action reminds one of the story of the Swiss muleteers, who are said to tie up their bells at certain places lest the tinkle should bring an avalanche down. The snow must be very delicately poised before this could occur. It probably never did occur, but our flame illustrates the principle. We bring it to the verge of falling, and the sonorous pulses precipitate what was already imminent.

When the flame flares, the gas in the orifice of the burner is in a state of vibration; conversely, when the gas in the orifice is thrown into vibration, the flame, if sufficiently near the flaring point, will flare. Thus the sonorous vibrations, by acting on the gas in the passage of the burner, become equivalent to an augmentation of pressure in the holder. In fact, we have here revealed to us the physical cause of flaring through excess of pressure, which, common as it is, has never been hitherto explained.

The gas encounters friction in the orifice of the burner, which, when the force of transfer is sufficiently great, throws the issuing stream into the state of vibration that produces flaring. It is because the flaring is thus caused that an infinitesimal amount of energy in the form of vibrations of the proper period can produce an effect equivalent to a considerable increase of pressure.

§ 10. *Seat of Sensitiveness.*

That the external vibrations act upon the gas in the orifice of the burner, and not, as some writers have supposed, upon the burner itself, the tube leading to it, or the

Fig. 136.

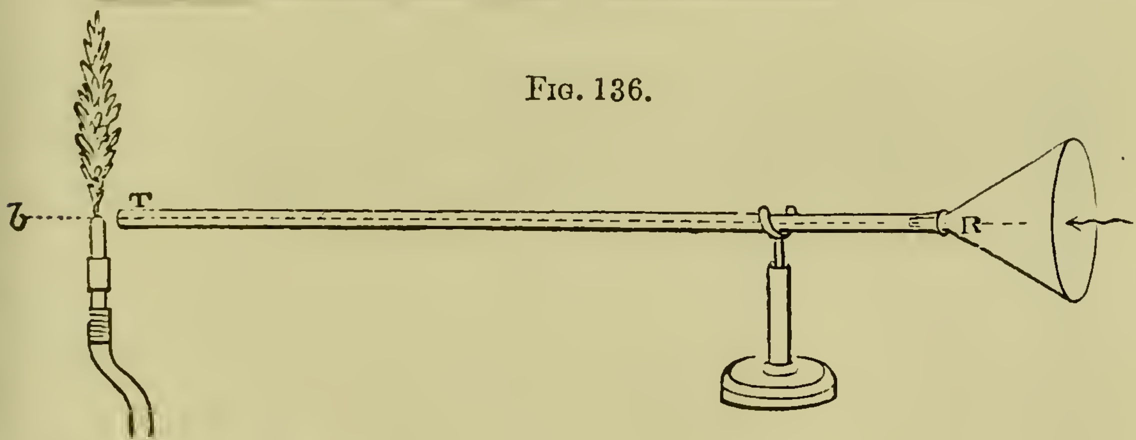


Fig. 137.

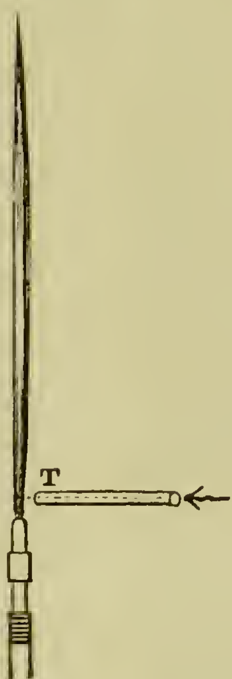


Fig. 138.

flame above it, is thus proved. A glass funnel R, fig. 136, is attached to a tube 3 feet long, and half an inch in diameter. A sensitive flame *b* is placed at the open end T of the tube, while a small high-pitched reed is placed in the funnel at R. When the sound is converged upon the root of the flame, as in fig. 136, the action is violent: when converged on a point half an inch above the burner, as in fig. 137, or at half an inch below the burner, as in fig. 138, there is no action. The glass tube may be dispensed with and the funnel alone employed, if care be taken to screen off all sound save that which passes through the shank of the funnel.[1]

§ 11. *Influence of Pitch.*

All sounds are not equally effective on the flame; waves of special periods are required to produce the maximum effect. The effectual periods are those which synchronise with the waves produced by the friction of the gas itself against the sides of its orifice. With a low-pressure flame a low deep whistle is more effective than a shrill one. With a high-pressure flame the exciting tremors must be very rapid, and the sound consequently shrill. Not one of these four tuning-forks, which vibrate 256 times, 320 times, 384 times, and 512 times respectively in a second, has any effect upon the flame from our iron nipple. But, besides their fundamental tones, these forks, as you know, can be caused to yield a series of overtones of very high pitch. The vibrations of this series are 1,600, 2,000, 2,400, and 3,200 per second, respectively. The flame jumps in response to each of these sounds; the response to that of the highest pitch being the most prompt and energetic of all.

To the tap of a hammer upon a board the flame re-

[1] In the actions described in the case of the blow-pipe and candle flames it was the jet of air issuing from the blow-pipe, and not the flame itself, that was directly acted on by the external vibrations.

sponds; but to the tap of the same hammer upon an anvil the response is much more brisk and animated. The reason is, that the clang of the anvil is rich in the higher tones to which the flame is most sensitive. The powerful tone obtained when our inverted bell is reinforced by its resonant tube has no power over this flame. But when a halfpenny is brought into contact with the vibrating surface the flame instantly shortens, flutters, and roars. I send an assistant with a smaller bell, worked by clock-work, to the most distant part of the gallery. He there detaches the hammer; the strokes follow each other in rhythmic succession, and at every stroke the flame falls from a height of 20 to a height of 8 inches, roaring as it falls.

FIG. 139.

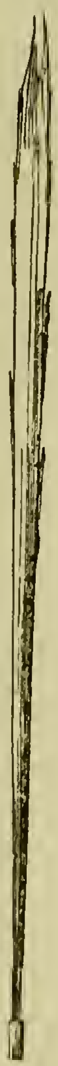


FIG. 140

The rapidity with which sound is propagated through air is well illustrated by these experiments. There is no sensible interval between the stroke of the bell and the ducking of the flame.

When the sound acting on the flame is of very short duration a curious and instructive effect is observed. The sides of the flame half-way down, and lower, are seen suddenly fringed by luminous tongues, the central flame remaining apparently undisturbed in both height and thickness. The flame in its normal state is shown in fig. 139, and with its fringes in fig. 140. The effect is due to the retention of the impression upon the retina. The flame actually falls as low as the fringes, but its recovery is so quick that to the eye it does not appear to shorten at all.[1]

[1] Numerous modifications of these experiments are possible. Other inflammable gases than coal gas may be employed. Mixtures of gases

§ 12. *The Vowel flame.*

I have now to introduce to your notice an astonishingly sensitive flame. It issues from the single orifice of a steatite burner, and reaches a height of 24 inches. The slightest tap on a distant anvil reduces its height to 7 inches. When a bunch of keys is shaken the flame is violently agitated, and emits a loud roar. The dropping of a sixpence into a hand already containing coin, at a distance of 20 yards, knocks the flame down. It is not possible to walk across the floor without agitating the flame. The creaking of boots sets it in violent commotion. The crumpling or tearing of paper, or the rustle of a silk dress, does the same. It is startled by the patter of a raindrop. I hold a watch near the flame: nobody hears its ticks; but you all see their effect upon the flame. At every tick it falls and roars. The winding up of the watch also produces tumult. The twitter of a distant sparrow shakes the flame down; the note of a cricket would do the same. A chirrup from a distance of 30 yards causes it to fall and roar. I repeat a passage from Spenser:—

> Her ivory forehead full of bounty brave,
> Like a broad table did itself dispread;
> For love his lofty triumphs to engrave,
> And write the battles of his great godhead.
> All truth and goodness might therein be read,
> For there their dwelling was, and when she spake,
> Sweet words, like dropping honey she did shed;
> And through the pearls and rubies softly brake
> A silver sound, which heavenly music seemed to make.

The flame selects from the sounds those to which it can respond. It notices some by the slightest nod, to others it bows more distinctly, to some its obeisance is very profound, while to many sounds it turns an entirely deaf ear.

have also been found to yield beautiful and striking results. An infinitesimal amount of mechanical impurity has been found to exert a powerful influence.

In fig. 141 this wonderful flame is represented. On chirruping to it, or on shaking a bunch of keys within a few yards of it, it falls to the size shown in fig. 142, the whole length, *a b*, of the flame being suddenly abolished. The light at the same time is practically destroyed, a pale and almost non-luminous residue of it alone remaining. These figures are taken from photographs of the flame.

Fig. 141.

To distinguish it from the others I have called this the 'vowel flame,' because the different vowel sounds affect it differently. A loud and sonorous U does not move the flame; on changing the sound to O, the flame quivers; when E is sounded, the flame is strongly affected. I utter the words *boot*, *boat*, and *beat* in succession. To the first there is no response; to the second, the flame starts; by the third is thrown into greater commotion. The sound *Ah!* is still more powerful. Did we not know the constitution of vowel sounds this deportment would be an insoluble enigma. As it is, however, the flame illustrates the theory of vowel sounds. It is most sensitive to sounds of high pitch; hence we should infer that the sound *Ah!* contains higher notes than the sound E; that E contains higher notes than O; and O higher notes than U. I need not say that this agrees perfectly with the analysis of Helmholtz.

Fig. 142.

This flame is peculiarly sensitive to the utterance of the letter S. A hiss contains the elements that most forcibly affect the flame. The gas issues from its burner with a hiss, and an external sound of this character is therefore exceedingly effective. From a metal box containing compressed air

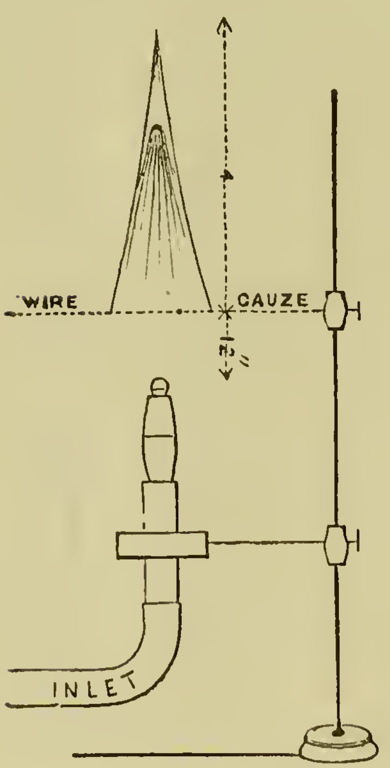
I allow a puff to escape; the flame instantly ducks down, not by any transfer of *air* from the box to the flame, for the distance between both utterly excludes this idea—it is the *sound* that affects the flame. From the most distant part of the gallery my assistant permits the compressed air to issue in puffs from the box; at every puff the flame suddenly falls. The pulses produced by the issuing air at the one orifice precipitate the tumult of the flame at the other.

When a musical-box is placed on the table, and permitted to play, the flame behaves like a sentient and motor creature—bowing slightly to some tones, and curtseying deeply to others.

§ 13. *Mr. Philip Barry's Sensitive Flame.*

Mr. Philip Barry has discovered a new and very effective form of sensitive flame which he thus describes in a letter to myself:—'It is the most sensitive of all the flames that I am acquainted with, though from its smaller size it is not so striking as your vowel-flame. It possesses the advantage that the ordinary pressure in the gas mains is quite sufficient to produce it. The method of producing it consists in igniting the gas (ordinary coal-gas) not at the burner but some inches above it, by interposing between the burner and the flame a piece of wire-gauze.

Fig. 143.



'I give a sketch of the arrangement adopted, fig. 143. The space between the burner and gauze was 2 inches. The gauze was about 7 inches square, resting on the ring of a re-

tort stand. It had 32 meshes to the lineal inch. The burner was Sugg's steatite pin-hole burner, the same as that used for the vowel-flame.

'The flame is a slender cone about four inches high, the upper portion giving a bright yellow light, the base being a non-luminous blue flame. At the least noise this flame roars, sinking down to the surface of the gauze, becoming at the same time invisible. It is very active in its responses, and being rather a noisy flame, its sympathy is apparent to the ear as well as the eye.

'To the vowel-sounds it does not appear to answer so discriminatingly as the vowel-flame. It is extremely sensitive to A, very slightly to E, more so to I, entirely non-sensitive to O, but slightly sensitive to U.

'It dances in the most perfect manner to a small musical snuff-box, and is highly sensitive to most of the sonorous vibrations which affect the vowel-flames.'

§ 14. *Lord Rayleigh's Sensitive Flame.*

Lord Rayleigh has recently devised a new arrangement for sensitive flames. A jet of coal gas from a pin-hole burner rises vertically in the interior of a cavity from which the air is excluded. It then passes into a brass tube, a few inches long, and on reaching the top, burns in the open. The front wall of the cavity is formed of a flexible membrane of tissue paper, through which external sounds can reach the burner.

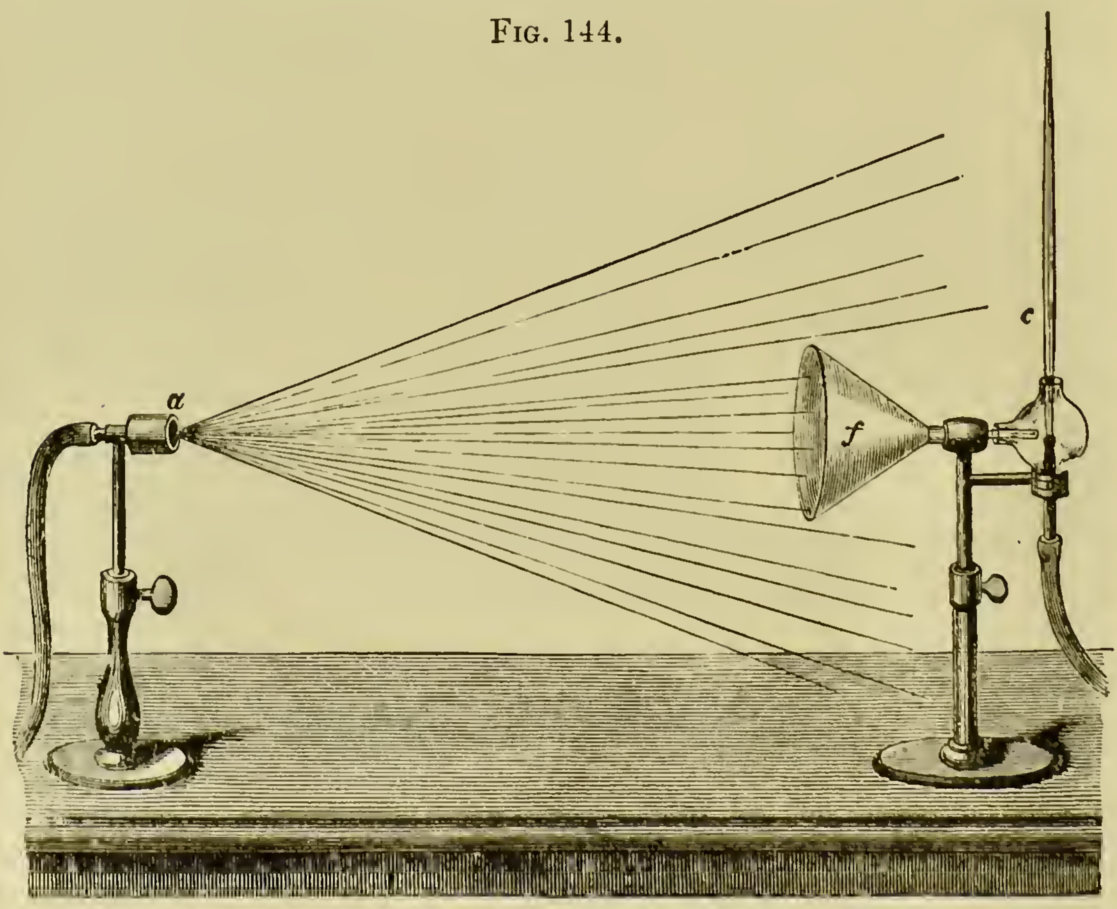
The principle is the same as that of Barry's flame. In both cases the *unignited* part of the jet is the sensitive agent and the flame is only an indicator. Barry's flame may be made very sensitive to sound, but it is open to the objection of liability to disturbance by the slightest draught. A few years since Mr. Ridout proposed to enclose the jet in a tube air-tight at the bottom, and to

ignite it only on arrival at the top of this tube. In this case, however, external vibrations have very imperfect access to the sensitive part of the jet, and when they reach it they are of the wrong quality, having but little motion transverse to the motion of the jet. Lord Rayleigh's arrangement combines very satisfactorily sensitiveness to sound and insensitiveness to wind, and it requires no higher pressure than that of ordinary gas pipes. If the extreme of sensitiveness be aimed at, the gas pressure must be adjusted until the jet is on the point of flaring without sound.

§ 15. *Converging and Diverging Sound Lenses.*

Before quitting the sensitive flame, I wish to make before you an instructive experiment on the refraction of

FIG. 144.

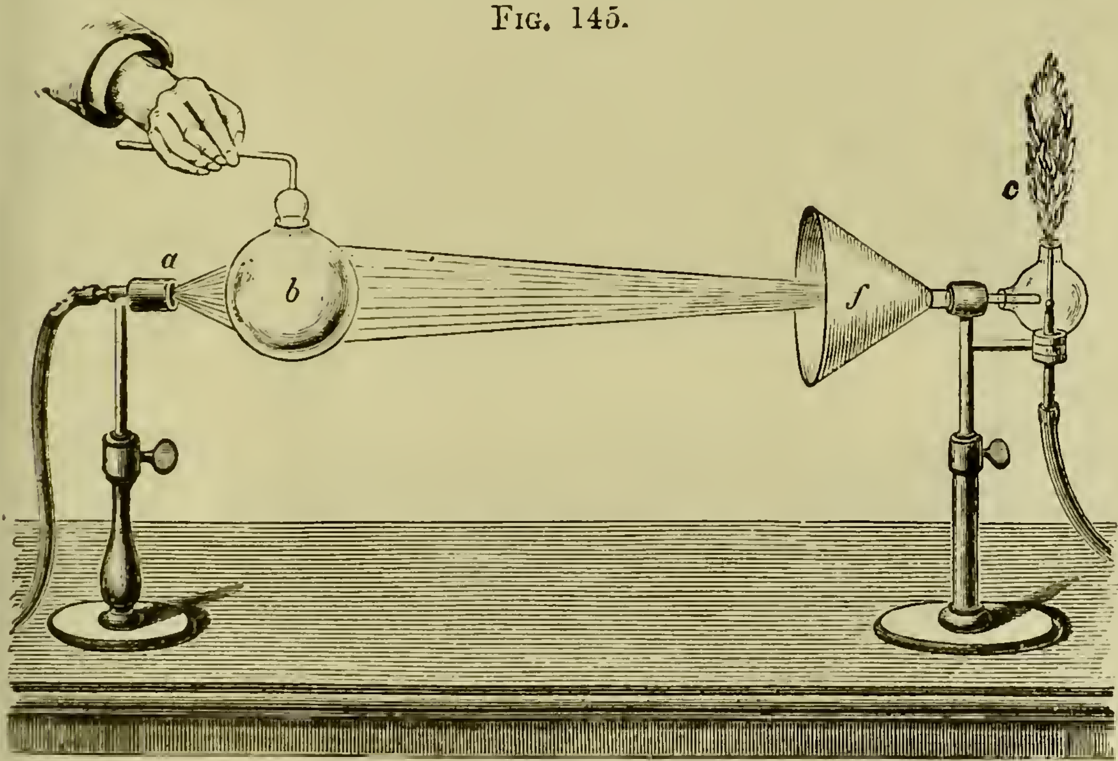


sound. This point has been already illustrated; but the experiment now to be made, instead of being confined to

the observation of a single person, will be apparent to you all. It will also extend the demonstration from converging to diverging sound lenses.

Nitrous oxide—the gas so much employed by dentists—possesses a density about equal to that of carbonic acid. This gas, which is far preferable to carbonic acid, I propose to employ for our converging sound lens. By pressing an india-rubber bag containing the gas, I produce a large soap bubble, which is intended to act as our lens. At *a*, fig. 144, is placed a concertina reed which produces a

Fig. 145.



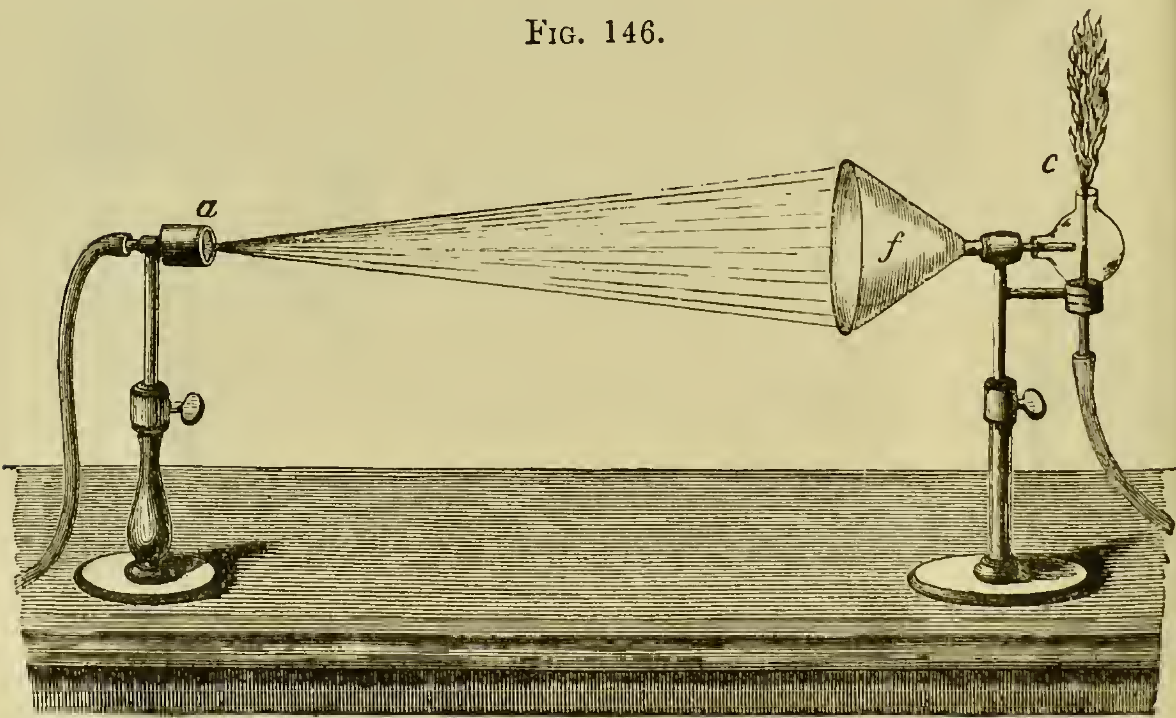
note of high pitch. At *c* is placed a sensitive flame. The pressure is so regulated that the flame burns steadily and tranquilly when the reed sounds. I place the soap bubble *b*, fig. 145, in front of the reed: the flame is thrown into stormy agitation by the convergence upon it of the rays of sound.[1] On removing the bubble the tranquillity of the flame is restored. I make the experiment several times in succession; always, when the bubble is absent,

[1] Represented by the lines drawn from the reed.

the flame burns steadily; always when the bubble is introduced it flares and roars. This action of the bubble on the waves of sound corresponds exactly with the action of a double convex glass lens upon the waves of light.

And now for the complementary experiment. Through nitrous oxide sound moves more slowly than through air; hence the power of this gas to concentrate the sonorous waves. Through hydrogen, as you know, sound passes much more rapidly than through air, and by a lens formed of this gas, the sonorous waves, instead of being concentrated, ought to be caused to diverge. Let us see

Fig. 146.

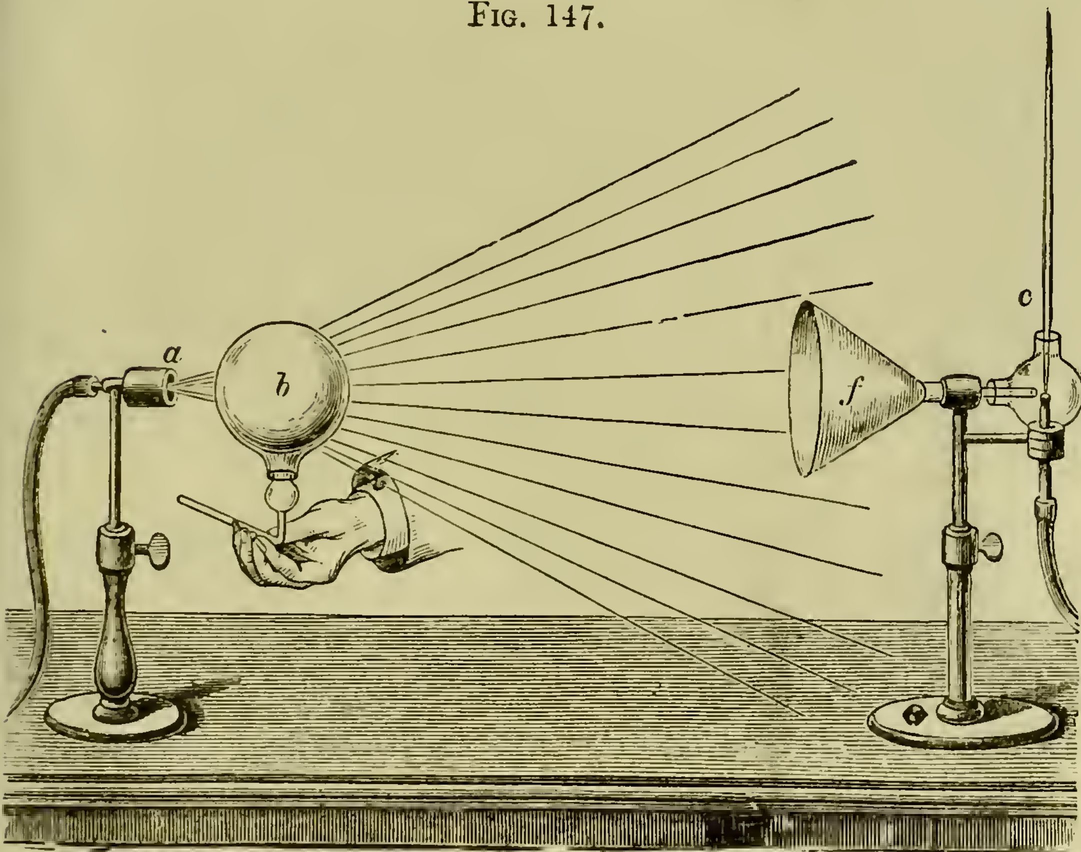


whether this is the case. As before, by means of Plateau's mixture, I blow a large bubble of hydrogen. At *a* (fig. 146) is our sounding reed, and at *c* our sensitive flame, the pressure being so regulated that the flame is thrown into violent agitation by the sound of the reed. I introduce the hydrogen bubble, *b* (fig. 147). The flame instantly rises tall and steady, and continues to burn tranquilly as long as the bubble is in front of the reed. This effect is due to the divergence of the waves of

sound. The action is precisely similar to that of a double concave lens upon the waves of light.

The sound lenses here employed are without wrinkles or corrugations of any kind. They are in this respect more perfect than the lenses employed by M. Sondhaus. They accept, moreover, and transmit with perfect pliancy the motion of the waves of sound. Nitrous oxide is

FIG. 147.



chosen in preference to carbonic acid, because the latter gas rapidly disintegrates the film and causes the bubble to burst. I may add that collodion balloons also make, with nitrous oxide and hydrogen, excellent lenses.

In the foregoing illustrations *f*, as before, represents a funnel the shank of which converges the sound waves upon the root of the flame.

§ 16. *Sensitive Smoke-jets.*

It is not to the flame, as such, that we owe the extraordinary phenomena which have been just described.

Effects substantially the same are obtained when a jet of unignited gas, of carbonic acid, hydrogen, or even air itself, issues from an orifice under proper pressure. None of these gases, however, can be seen in its passage through air, and, therefore, we must associate with them some substance which, while sharing their motions, will reveal them to the eye. The method employed from time to time in this place of rendering aerial vortices visible has been known to some of you for half a century. By tapping a membrane which closes the mouth of a large funnel filled with smoke, we obtain beautiful smoke-rings, which reveal the motion of the air. By associating smoke with our gas-jets, in the present instance, we can also trace their course, and when this is done, the unignited gas proves as sensitive as the flames. The smoke-jets jump, shorten, split into forks, or lengthen into columns, when the proper notes are sounded.

Underneath this gas-holder are placed two small basins, the one containing hydrochloric acid, and the other ammonia. Fumes of sal-ammoniac are thus copiously formed, and mingle with the gas contained in the holder. We may, as already stated, operate with coal-gas, carbonic acid, air, or hydrogen; each of them yields good effects. From our excellent steatite burner now issues a thin column of smoke. On sounding the whistle, which was so effective with the flames, it is found ineffective. When, moreover, the highest notes of a series of Pandean pipes are sounded, they are also ineffective. Nor will the lowest notes answer. But when a certain pipe, which stands about the middle of the series, is sounded, the smoke-column falls, forming a short stem with a thick bushy head. It is also pressed down, as if by a vertical wind, by a knock upon the table. At every tap it drops. A stroke on an anvil, on the contrary, produces little or no effect. In fact, the pressure being small, the notes here effective

are of a much lower pitch than those which were most efficient in the case of the flames.

The amount of shrinkage exhibited by some of these smoke-columns, in proportion to their length, is far greater than that of the flames. A tap on the table causes a smoke-jet eighteen inches high to shorten to a bushy bouquet with a stem not more than an inch in height. The smoke-column, moreover, responds to the voice. A cough knocks it down; and it dances to the tune of a musical-box. Some notes cause the mere top of the column to gather itself up into a bunch; at other notes the bunch is formed midway down; while notes of more suitable pitch cause the column to contract itself to a cumulus not much more than an inch above the end of the burner. Various forms of the dancing smoke-jet are shown in fig. 148. As the music continues, the action of the smoke-column consists of a series of rapid leaps from one of these forms to another.

Fig. 148.

In a perfectly still atmosphere these slender smoke-columns rise sometimes to a height of nearly two feet, apparently vanishing into air at the summit. When this is the case, our most sensitive flames fall far behind them in delicacy; and though less striking than the flames, the smoke-wreaths are often more graceful. Not only special words, but every word, and even every syllable of the foregoing stanza from Spenser, tumbles a really sensitive

smoke-jet into confusion. To produce such effects, a perfectly tranquil atmosphere is necessary. Flame experiments, in fact, are possible in an atmosphere where smoke-jets are utterly unmanageable.[1]

§ 17. *Constitution of Liquid Veins: Sensitive Water-jets.*

We have thus far confined our attention to jets of ignited and unignited coal-gas—of carbonic acid, hydrogen, and air. We will now turn to jets of water. And here a series of experiments, remarkable for their beauty, has long existed, which claim relationship to those just described. These are the experiments of Felix Savart on liquid veins. If the bottom of a vessel containing water be pierced by a circular orifice, the descending liquid vein will exhibit two parts unmistakably distinct. The part of the vein nearest the orifice is steady and limpid, presenting the appearance of a solid glass rod. It decreases in diameter as it descends, reaches a point of maximum contraction, from which point downwards it appears turbid and unsteady. The course of the vein, moreover, is marked by periodic swellings and contractions. Savart has represented these appearances as in fig. 149. The part *a n* nearest the orifice is limpid and steady, while all the part below *n* is in a state of quivering motion. This lower part of the vein appears continuous to the eye; but the finger can be sometimes passed through it without being wetted. This, of course, could not be the case if the vein were really continuous.

[1] Referring to these effects, Helmholtz says:—'Die erstaunliche Empfindlichkeit eines mit Rauch imprägnirten cylindrischen Luftstrahls gegen Schall ist von Herrn Tyndall beschrieben worden; ich habe dieselbe bestätigt gefunden. Es ist dies offenbarr eine Eigenschaft der Trennungsflächen die für das Anblasen der Pfeifen von grösster Wichtigkeit ist.' Discontinuirliche Luftbewegung, Monatsbericht, April 1868.

The upper portion of the vein, moreover, intercepts vision; the lower portion, even when the liquid is mercury, does not. In fact the vein resolves itself, at *n*, into liquid spherules, its apparent continuity being due to the retention of the impressions made by the falling drops upon the retina. If, while looking at the disturbed portion of the vein, the head be suddenly lowered, the descending column will be resolved for a moment into separate drops. One way of reducing the vein to its constituent spherules is to illuminate the vein, in a dark room, by a succession of electric flashes. Every flash reveals the drops, as if they were perfectly motionless in the air.

Could the appearance of the vein illuminated by a single flash be rendered permanent, it would be that represented in fig. 150. And here we find revealed the cause of those swellings and contractions which the disturbed portion of the vein exhibits. The drops, as they descend, are continually changing their forms. When first

Fig. 149.

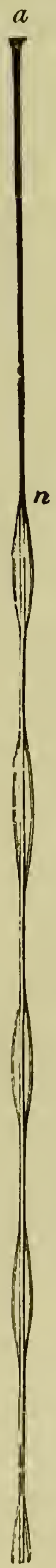


Fig. 150.

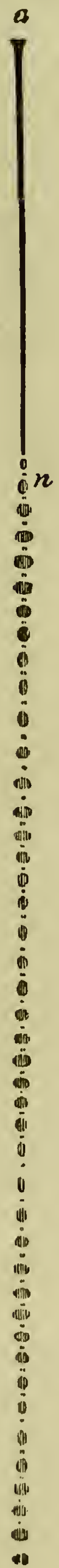


Fig. 151.

detached from the end of the limpid portion of the vein, the drop is a spheroid with its longest axis vertical. But a liquid cannot retain this shape, if abandoned to the forces of its own molecules. The spheroid seeks to become a sphere—the longer diameter therefore shortens; but, like a pendulum which seeks to return to its position of rest, the contraction of the vertical diameter goes too far, and the drop becomes a flattened spheroid. Now, the contractions of the jet are formed at those places where the longest axis of the drop is vertical, while the swellings appear where the longest axis is horizontal. It will be noticed that between every two of the larger drops is a third one of much smaller dimensions. According to Savart, their appearance is invariable.

I wish to make the constitution of a liquid vein evident to you by a simple but beautiful experiment. The condensing lens has been removed from our electric lamp, the light being permitted to pass directly through a vertical slit fixed in front of the camera. The slice of light thus obtained is so divergent that it illuminates, from top to bottom, a liquid vein several feet long, placed at some distance from the lamp. Immediately in front of the camera is a large disc of zinc with six radial slits, about ten inches long and an inch wide. By the rotation of the disc the light is caused to fall in flashes upon the jet; and when the suitable speed of rotation has been attained, the vein is resolved into its constituent spherules. Receiving the shadow of the vein upon a white screen, its constitution is rendered clearly visible to all here present.

This breaking-up of a liquid vein into drops has been a subject of frequent experiment and much discussion. Savart traced the pulsations to the orifice, but he did not think that they were produced by friction. They are powerfully influenced by sonorous vibrations. In the

midst of a large city it is hardly possible to obtain the requisite tranquillity for the full development of the continuous portion of the vein. Savart was so far able to withdraw his vein from the influence of such irregular vibrations, that its limpid portion became elongated to the extent shown in fig. 151. It will be understood that fig. 149 represents a vein exposed to the irregular vibrations of the city of Paris, while fig. 151 represents one produced under precisely the same conditions, but withdrawn from those vibrations.

The drops into which the vein finally resolves itself, are incipient even in its limpid portion, announcing themselves there as annular protuberances, which become more and more pronounced, until finally they separate. Their birth-place is near the orifice itself, and under even moderate pressure they succeed each other with sufficient rapidity to produce a feeble musical note. By permitting the drops to fall upon a membrane, the pitch of this note may be fixed; and now we come to the point which connects the phenomena of liquid veins with those of sensitive flames and smoke-jets. If a note in unison with that of the vein be sounded near it, the limpid portion instantly shortens. The pitch may vary to some extent, and still cause a shortening; but the unisonant note is the most effectual. Savart's experiments on vertically descending veins have been recently repeated in our laboratory with striking effect. From a distance of thirty yards the limpid portion of the vein has been shortened by the sound of an organ-pipe of the proper pitch and of moderate intensity.

In Plateau's researches on the figures of equilibrium assumed by bodies withdrawn from the action of gravity, he finds that a liquid cylinder is stable as long as its length does not exceed three times its diameter; or, more accurately, as long as the ratio between them does not exceed

that of the diameter of a circle to its circumference, or 3·1416. If this be a little exceeded the cylinder begins to narrow at some point or other of its length: nips itself there breaks, and forms immediately two spheres. If the ratio of the length of the cylinder to its diameter greatly exceed 3·1416, then, instead of breaking up into two spheres, it breaks up into several.

A liquid cylinder may be obtained by introducing olive oil into a mixture of alcohol and water, of the same density as the oil. The oil forms a sphere. Two discs of smaller diameter than the sphere are brought into contact with it, and then drawn apart; the oil clings to the discs, and the sphere is transformed into a cylinder. If the quantity of oil be insufficient to produce the maximum length of cylinder, more may be added by a pipette. In making this experiment it will be noticed that when the proper length is exceeded, the nipped portion of the cylinder elongates, and exists for a moment as a very thin liquid cylinder uniting the two incipient spheres; and that when rupture occurs, the thin cylinder, which has also exceeded *its* proper length, breaks so as to form a small spherule between the two larger ones. This is a point of considerable significance in relation to our present question.

Now, Plateau contends that the play of the molecular forces in a liquid cylinder is not suspended by its motion of translation. The first portion of a vein of water quitting an orifice is a cylinder, to which the laws which he has established regarding motionless cylinders apply. The moment the descending vein exceeds the proper length it begins to pinch itself so as to form drops; but urged forward as it is by the pressure above it, and by its own gravity, in the time required for the rounding of the drop it reaches a certain distance from the orifice. At this distance, the pressure remaining constant, and the vein being withdrawn from external disturbance, rupture in-

variably occurs. And the rupture is accompanied by the phenomenon which has been just called significant. Between every two succeeding large drops a small spherule is formed as shown in fig. 150.

Permitting a vein of oil to fall from an orifice, not through the air, but through a mixture of alcohol and water of the proper density, the continuous portion of the vein, its resolution into drops, and the formation of the small spherule between each liberated drop and the end of the liquid cylinder which it has just quitted, may be watched with the utmost deliberation. The effect of this and other experiments upon the mind will be to produce the conviction that the very beautiful explanation offered by Plateau is also the true one. The various laws established experimentally by Savart all follow immediately from Plateau's theory.

In a small paper published many years ago I drew attention to the fact that when a descending vein intersects a liquid surface above the point of rupture, if the pressure be not too great, it enters the liquid *silently*; but when the surface intersects the vein below the point of rupture a rattle is immediately heard, and bubbles are copiously produced. In the former case, not only is there no violent dashing aside of the liquid, but round the base of the vein, and in opposition to its motion, the liquid collects in a heap, by its surface tension or capillary attraction. This experiment can be combined with two other observations of Savart's, in a beautiful and instructive manner. In addition to the shortening of the continuous portion by sound, Savart found that when he permitted his membrane to intersect the vein at one of its protuberances, the sound was louder than when the intersection occurred at the contracted portion.

I now permit a vein to descend, under scarcely any pressure, from a tube three-quarters of an inch in dia-

meter, and enter silently a basin of water placed nearly 20 inches below the orifice. On sounding vigorously a tuning-fork, vibrating 128 times in a second, the pellucid jet is instantly broken, and as many as three of its swellings rise above the surface. The rattle of air bubbles is instantly heard, the basin being filled with them. As the sound slowly dies out, the continuous portion of the vein lengthens, a series of alternations in the production of the bubbles being observed. When the swellings of the vein cut the surface of the water, the bubbles are copious and loud; when the contracted portion crosses the surface, the bubbles are scanty and scarcely audible. Finally the vein, as the sound dies away, assumes its original continuity and steadiness.

Removing the basin, placing an iron tray in its place, and exciting the fork, the vein, which at first strikes silently upon the tray, commences a rattle which rises and sinks with the dying out of the sound, according as the swellings or contractions of the jet impinge upon the surface. These are simple and beautiful experiments.

Savart also caused his vein to issue not only vertically, but horizontally and at various inclinations to the horizon. He found that, in certain cases, sonorous vibrations were competent to cause a jet to divide into two or three branches. In these experiments the liquid was permitted to issue through an orifice in a thin plate. Instead of this, however, we will resort to our favourite steatite burner; for with water also it asserts the same mastery over its fellows that it exhibited with flames and smoke-jets. It will, moreover, reveal to us some entirely novel results.

By means of an india-rubber tube the burner is connected with the water-pipes of the Institution, and, by pointing it obliquely upwards, we obtain a fine parabolic jet (fig. 152). At a certain distance from the orifice, the

vein resolves itself into beautiful spherules, whose motions are not rapid enough to make the vein appear continuous. At the vertex of the parabola the spray of pearls is more than an inch across, and, further on, the drops are still more widely scattered. On sweeping a fiddle-bow across a

Fig. 152.

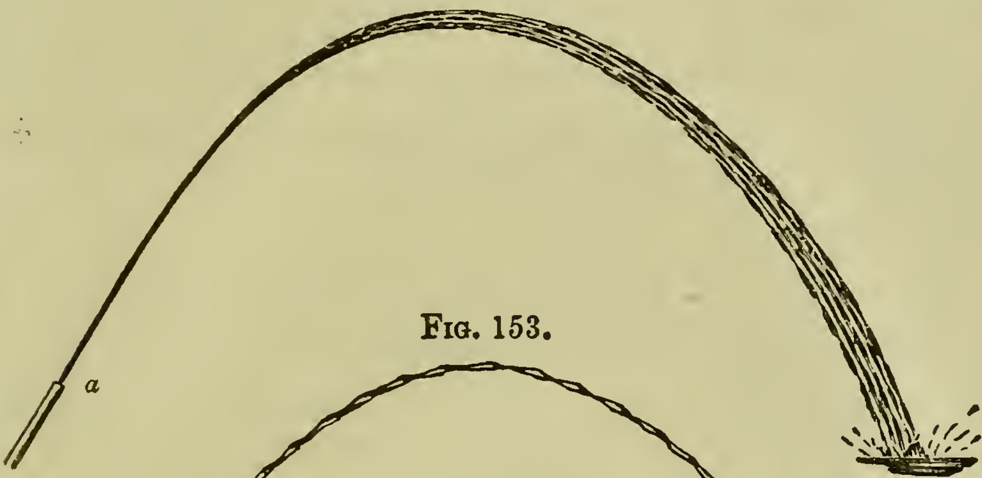


Fig. 153.

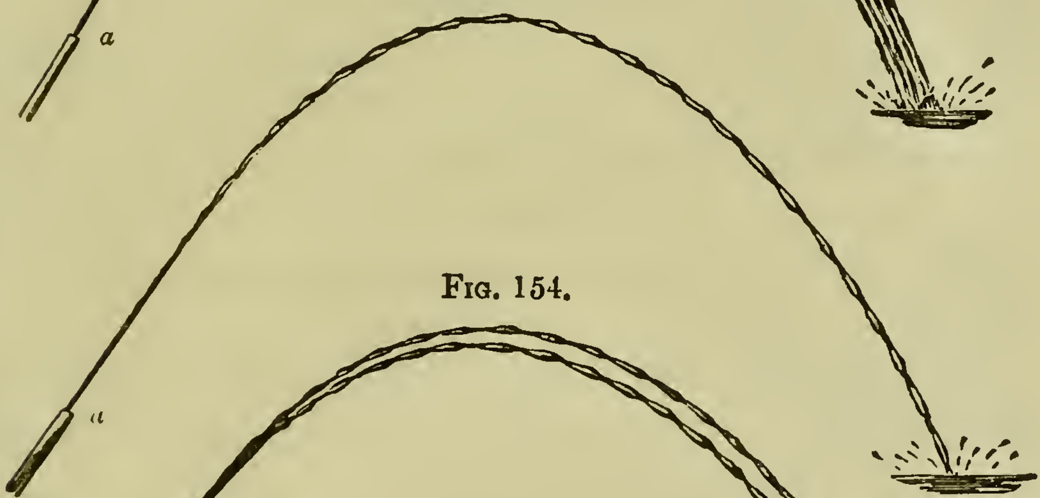
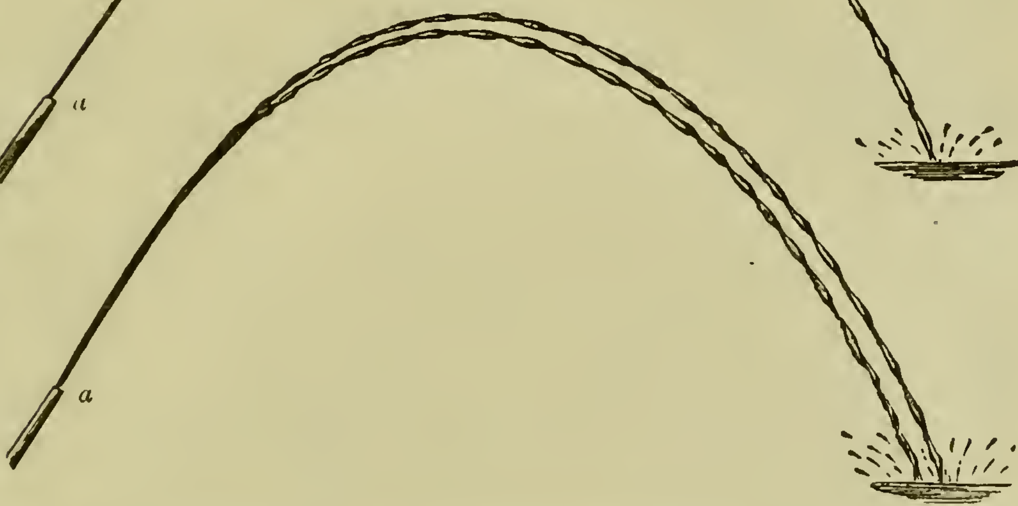


Fig. 154.



tuning-fork which executes 512 vibrations in a second, the scattered drops, as if drawn together by their mutual attractions, instantly close up, and form an apparently continuous liquid arch several feet in height and span (shown in fig. 153). As long as the proper note is maintained the vein looks like a frozen band, so motionless does it appear. On stopping the fork the arch is shaken

asunder, and we have the same play of liquid pearls as before. Every sweep of the bow, however, causes the drops to fall into a common line of march.

A pitch-pipe, or an organ-pipe yielding the note of this tuning-fork, also powerfully controls the vein. The voice does the same. On pitching it to a note of moderate intensity, it causes the wandering drops to gather themselves together. At a distance of twenty yards, the voice is, to all appearance, as powerful in curbing the vein, and causing its drops to close up, as it is when close to the issuing jet.

By means of a circle of zinc cut into radial slits, and caused to rotate rapidly, a succession of flashes may be thrown upon the vein. Its resolution into drops is thus rendered strikingly evident, while the formation and arrangement of new drops at the end of the continuous portion of the vein, when the proper musical note is sounded, can be seen by every member of this large audience. The experiment is one of extreme beauty.

The effect of 'beats' upon the vein is also beautiful and instructive. They may be produced either by organ-pipes or by tuning-forks. You will learn in our next lecture that when two forks vibrate, the one 512 times, and the other 508 times in a second, they produce four beats in a second. When these forks are sounded the beats are heard, and the liquid vein is seen to gather up its pearls, and scatter them in synchronism with the beats. The sensitiveness of this vein is astounding; it rivals that of the ear itself. Placing the two tuning-forks on a distant table, and permitting the beats to die gradually out, the vein continues its rhythm almost as long as hearing is possible. A more sensitive vein might actually prove superior to the ear—a very surprising result, considering the marvellous delicacy of this organ.[1]

[1] When these two tuning-forks were placed *in contact* with a vessel

By introducing a Leyden jar into the circuit of a powerful induction-coil, a series of dense and dazzling flashes of light, each of momentary duration, is obtained. Every such flash in a darkened room renders the drops distinct, each drop being transformed into a little star of intense brilliancy. If the vein be then acted on by a sound of the proper pitch, it instantly gathers its drops together into a necklace of inimitable beauty.

In these experiments the whole vein gathers itself into a single arched band when the proper note is sounded; but, by varying the conditions, it may be caused to divide into two or more such bands, as shown in fig. 154. Drawings, however, are ineffectual here; for the wonder of these experiments depends mainly on the sudden transition of the vein from one state to the other. In the *motion* dwells the surprise, and this no drawing can render.[1]

from which a liquid vein issued, the visible action on the vein continued long after the forks had ceased to be heard.

[1] The experiments on sounding flames have been considerably extended, at my request, by my assistant Mr. Cottrell. By causing flame to rub against flame, various musical sounds can be obtained—some resembling those of a trumpet, others those of a lark. By the friction of unignited gas-jets, similar though less intense effects are produced. When the two flames of a fish-tail burner are permitted to impinge on a plate of platinum, as in Scholl's 'perfectors,' the sounds are trumpet-like, and very loud. Two ignited gas-jets may be caused to flatten out like Savart's water-jets. Or they may be caused to roll themselves into two hollow horns, forming a most instructive example of the *Wirbelflächen* of Helmholtz. The carbon particles liberated in the flame rise with the horns in continuous red-hot or white-hot spirals, which are extinguished at a height of some inches from their place of generation.

SUMMARY OF LECTURE VI.

SINGING FLAMES.

When a gas-flame is placed in a tube, the air in passing over the flame is thrown into vibration, musical sounds being the consequence.

Making allowance for the high temperature of the column of air associated with the flame, the pitch of the note is that of an open organ-pipe of the length of the tube surrounding the flame.

The vibrations of the flame, while the sound continues, consists of a series of periodic extinctions, total or partial, between every two of which the flame partially recovers its brightness.

The periodicity of the phenomenon may be demonstrated by means of a concave mirror which forms an image of the vibrating flame upon a screen. When the image is sharply defined, the rotation of the mirror reduces the single image to a series of separate images of the flame. The dark spaces between the images correspond to the extinctions of the flame, while the images themselves correspond to its periods of recovery.

Besides the fundamental note of the associated tube, the flame can also be caused to excite the higher overtones of the tube. The successive divisions of the column of air are those of an open organ-pipe when its harmonic tones are sounded.

On sounding a note nearly in unison with a tube containing a silent flame, the flame jumps ; and if the position

of the flame in the tube be rightly chosen, the extraneous sound will cause the flame to sing.

While the flame is singing, a note nearly in unison with its own produces beats, and the flame is seen to jump in synchronism with the beats. The jumping is also observed when the position of the flame within its tube is not such as to enable it to sing.

SENSITIVE FLAMES.

When the pressure of the gas which feeds a naked flame is augmented, the flame, up to a certain point, increases in size. But if the pressure be too great, the flame roars or flares.

The roaring or flaring of the flame is caused by the state of vibration into which the gas is thrown in the orifice of the burner, when the pressure which urges it through the orifice is excessive.

If the vibrations in the orifice of the burner be superinduced by an extraneous sound, the flame will flare under a pressure less than that which, of itself, would produce flaring.

The gas under excessive pressure has vibrations of a definite period impressed upon it as it passes through the burner. To operate with a maximum effect upon the flame the external sound must contain vibrations synchronous with those of the issuing gas.

When such a sound is chosen, and when the flame is brought sufficiently near its flaring point, it furnishes an acoustic reagent of unexampled delicacy.

At a distance of 30 yards, for example, the chirrup of a house sparrow would be competent to throw the flame into commotion.

By means of soap bubbles blown with nitrous oxide on the one hand, and with hydrogen on the other, the

divergence and the convergence of the waves of sound may be illustrated in a beautiful and striking manner. By the converging sound-lens a tranquil sensitive flame is thrown into violent agitation. By the diverging sound-lens a violently agitated flame is rendered perfectly tranquil. The action of double convex and double concave glass lenses upon light is thus perfectly imitated in the case of sound.

It is not to the flame, as such, that we are to ascribe these effects. Effects substantially similar are produced when we employ jets of unignited coal-gas, carbonic acid, hydrogen, or air. These jets may be rendered visible by smoke, and the smoke-jets show a sensitiveness to sonorous vibrations even greater than that of the flames.

When a brilliant sensitive flame illuminates an otherwise dark room, in which a suitable bell is caused to strike, a series of periodic quenchings of the light by the sound occurs. Every stroke of the bell is accompanied by a momentary darkening of the room.

A jet of water descending from a circular orifice is composed of two distinct portions, the one pellucid and calm; the other in commotion. When properly analysed the former portion is found continuous; the latter being a succession of drops.

If these drops be received upon a membrane, a musical sound is produced. When an extraneous sound of this particular pitch is produced in the neighbourhood of the vein, the continuous portion is seen to shorten.

The discontinuous portion of the vein presents a series of swellings and contractions, in the former of which the drops are flattened, and in the latter elongated. The sound produced by the flattened drops on striking the membrane, is louder than that produced by the elongated ones.

Above its point of rupture a vein of water may be

caused to enter water *silently*; but on sounding a suitable note, the rattle of bubbles is immediately heard; the discontinuous part of the vein rises above the surface, and as the sound dies out the successive swellings and contractions produce alternations of the quantity and sound of the bubbles.

In veins propelled obliquely, the scattered water-drops may be called together by a suitable sound, so as to form an apparently continuous liquid arch.

Liquid veins may be analysed by the electric spark, or by a succession of flashes illuminating the veins.

LECTURE VII.

PART I.

RESEARCHES ON THE ACOUSTIC TRANSPARENCY OF THE ATMOSPHERE IN RELATION TO THE QUESTION OF FOG-SIGNALLING.

INTRODUCTION—INSTRUMENTS AND OBSERVATIONS—INFLUENCE OF SOUND-SHADOW—CONTRADICTORY RESULTS—SOLUTION OF CONTRADICTIONS—AERIAL REFLECTION AND ITS CAUSES—AERIAL ECHOES—ACOUSTIC CLOUDS—EXPERIMENTAL DEMONSTRATION OF STOPPAGE OF SOUND BY AERIAL REFLECTION.

§ 1. *Introduction.*

THE care of its sailors is one of the first duties of a maritime people, and one of the sailor's greatest dangers is his proximity to the coast at night. Hence the idea of warning him of such proximity by beacon-fires placed sometimes on natural eminences and sometimes on towers built expressly for the purpose. Close to Dover Castle, for example, stands an ancient Pharos of this description.

As our marine increased greater skill was invoked, and lamps reinforced by parabolic reflectors poured their light upon the sea. Several of these lamps were sometimes grouped together so as to intensify the light, which at a little distance appeared as if it emanated from a single source. This 'catoptric' form of apparatus is still to some extent employed in our lighthouse service, but for a long time past it has been more and more displaced by the great lenses devised by the illustrious Frenchman, Fresnel.

In a first-class 'dioptric' apparatus the light emanates from a lamp with several concentric wicks, the flame of which, being kindled by a very active draught, attains to great intensity. In fixed lights the lenses refract the rays issuing from the lamp so as to cause them to form a luminous sheet which grazes the sea-horizon. In revolving lights the lenses gather up the rays into distinct beams, resembling the spokes of a wheel, which sweep over the sea and strike the eye of the mariner in succession.

It is not for clear weather that the greatest strengthening of the light is intended, for here it is not needed. Nor is it for densely foggy weather, for here it is ineffectual. But it is for the intermediate stages of hazy, snowy, or rainy weather, in which a powerful light can assert itself, while a feeble one is extinguished. The usual first-order lamp is one of four wicks, but Mr. Douglass (now Sir James Douglass), the able and indefatigable engineer of the Trinity House, had the number of the wicks raised to six, which produce a very noble flame.[1] The six-wick lamp was preceded in time and surpassed in power by the burners of Mr. Wigham, of Dublin, to whom we are indebted for the successful application of gas to lighthouse illumination. In some lighthouses his power varies from 28 jets to 108 jets, while in the lighthouse of Galley Head a power of 432 jets could, if desired, be employed. These larger powers are invoked only in case of fog, the 28-jet burner being amply sufficient for clear weather. The passage from the small burner to the large, and from the large burner to the small, is made with ease, rapidity, and certainty. This employment of gas is indigenous to Ireland, and the Board of Trade has exercised a wise liberality in allowing every facility to Mr. Wigham for the development of his invention.

[1] An 8-wick lamp has been recently produced under the superintendence of the engineer of the Trinity House.

The last great agent employed in lighthouse illumination is electricity. It was in this Institution, beginning in 1831, that Faraday proved the existence and illustrated the laws of those induced currents which in our day have received such astounding development. In relation to this subject Faraday's words have a prophetic ring: 'I have rather,' he writes in 1831, 'been desirous of discovering new facts and new relations dependent on magneto-electric induction than of exalting the force of those already obtained, being assured that the latter would find their full development hereafter.' The labours of modern electricians constitute a brilliant fulfilment of this prediction.

Our most intense lights, including the six-wick lamp, the Wigham gas-light, and the electric light, being intended to aid the mariner in heavy weather, may be regarded, in a certain sense, as fog signals. But the cloud produced by the puff of a locomotive can quench the rays of the noonday sun; it is not therefore surprising that in dense fogs our most powerful coast-lights, including even the electric light, should become useless to the mariner.

Disastrous shipwrecks are the consequence. During the ten years ending in 1874 no less than two hundred and seventy-three vessels were reported as totally lost on our own coasts in fog or thick weather. The loss, I believe, is far greater on the American seaboard, where trade is more eager and fogs more frequent than they are here. No wonder, then, that earnest efforts should have been made to find a substitute for light in sound-signals, powerful enough to give warning and guidance to mariners while still at a safe distance from the shore.

Such signals have been established to some extent upon our own coasts, and to a still greater extent along

the coasts of Canada and the United States. But the evidence as to their value and performance was of the most conflicting character, and no investigation sufficiently thorough to clear up uncertainty and explain conflicting observations had been made. Soon after my return from America in 1873 I was requested by the Elder Brethren of the Trinity House to undertake the direction of an inquiry which should fill the blank here indicated. I entered upon it inspired by duty rather than hope, for I feared that the observations would be tedious and the scientific results uncertain. But the study of any natural problem, if only steadfastly pursued, is sure in the end to reward the inquirer. And so in the present instance, after some preliminary groping, light began to dawn upon the subject, exposing many old errors and revealing some novel truths.

§ 2. *Condition of the Question.*

In a very clear and able letter addressed to the President of the Board of Trade in 1863,[1] Dr. Robinson, of Armagh, thus summarises our knowledge of fog-signals :—' Nearly all that is known about fog-signals is to be found in the Report on Lights and Beacons ; and of it much is little better than conjecture. Its substance is as follows :—

' Light is scarcely available for this purpose. Blue lights are used in the Hoogly ; but it is not stated at what distance they are visible in fog : their glare may be seen further than their flame. It might, however, be desirable to ascertain how far the electric light or its flash can be traced.[2]

' Sound is the only known means really effective ; but about it testimonies are conflicting, and there is scarcely

[1] Report of the British Association for 1863, p. 105.

[2] Powerful electric lights have been since established on the coast of England.

one fact relating to its use as a signal which can be considered as established. Even the most important of all, the distance at which it ceases to be heard, is undecided.

'Up to the present time all signal-sounds have been made in air, though this medium has grave disadvantages: its own currents interfere with the sound-waves, so that a gun or bell which is heard several miles *down* the wind is inaudible more than a few furlongs *up* it. A still greater evil is that it is least effective when most needed; for fog is a powerful damper of sound.'

Dr. Robinson here expresses the prevalent opinion, and he then assigns the theoretic cause of the acoustic opacity of fog. Fog, he says, 'is a mixture of air and globules of water, and at each of the innumerable surfaces where these two touch, a portion of the vibration is reflected and lost.[1] . . . . . Snow produces a similar effect, and one still more injurious.'

Reflection being thus considered to take place at the surfaces of the suspended particles, it followed that the greater the number of particles, or, in other words, the denser the fog, the more injurious would be its action upon sound. Hence optical transparency came to be considered as a measure of acoustic transparency. On this point Dr. Robinson, in the letter referred to, expresses himself thus:—'At the outset, it is obvious that, to make experiments *comparable*, we must have some measure of the fog's power of stopping sound, without attending to which the most anomalous results may be expected. It seems probable that this will bear some simple relation to its opacity to light, and that the distance at which a given object, as a flag or pole, disappears, may be taken as the measure.' 'Still clear air' is regarded in this letter as the best vehicle of sound, the alleged action of fogs, rain, and

[1] This is also Sir John Herschel's way of regarding the subject (Essay on Sound, par. 38).

snow being ascribed to their rendering the atmosphere 'a discontinuous medium.'

To Mr. Alexander Beazeley we are indebted for an extremely useful summary of existing knowledge regarding fog signals.[1] He classifies the various instruments hitherto employed, and gives some account of their performance. As regards the action of fog upon sound, the statements made in the body of his papers agree with those just quoted from Dr. Robinson. 'Fogs,' he says, 'have a remarkable power of deadening sound, and act in this respect so irregularly that experiments made during clear weather have little or no practical value, except as mere competitive trials of different instruments.'

In the discussion which followed the reading of Mr. Beazeley's paper at the Institution of Civil Engineers, Dr. Gladstone, who had been a member of the Commission on Lights and Beacons, is reported to have said: 'A difficulty in the use of sound was this, that fogs deaden sound very materially; but the evidence was very contradictory on that point. In a fog on land it was difficult to hear the passing of carriages or noises at a short distance: and so in a fog at sea these signals found a difficulty in penetrating the fog against which they are intended to be a protection.'

On the same occasion Mr. James N. Douglass, the Engineer of the Trinity House, to whose ability as an observer I am able to bear strong testimony, stated that in his experience 'he had found but little difference in the travelling of sound in foggy or in clear weather. He had distinctly heard in a fog, at the Small's Rock in the Bristol Channel, guns fired at Milford, twenty-five miles off.' Mr. Beazeley had also heard the Lundy Island gun 'at Hartland Point, a distance of ten miles, during

[1] Proceedings of the Institution of Civil Engineers, March 14, 1871, and Lecture at the United Service Institution, May 24, 1872.

dense fog'; so that, in winding up his paper, he admitted 'that the subject appeared to be very little known, and that the more it was looked into the more apparent became the fact that the evidence as to the action of fog upon sound is extremely conflicting.'

In a paper presented to the Literary and Philosophical Society of Manchester on the 16th of December, 1873, Professor Osborne Reynolds affirmed, with great distinctness, the prevalent doctrine, which he shared, and made a very ingenious attempt to explain it. 'That sound,' says Professor Reynolds, 'does not readily penetrate a fog is a matter of common observation. The bells and horns of ships are not heard so far during fogs as when the weather is clear. In a London fog the noise of the wheels is much diminished, so that they seem to be at a distance when really close by.' Professor Reynolds does not accept reflection at the surfaces of the particles as sufficient to explain the opacity of fogs to sound. He ascribes the loss to the friction set up between the air and the foreign particles suspended in it. 'The effect,' he says, 'of waves of sound traversing a portion of air is first to accelerate and then to retard it. And if there are any drops of water in the air, these will not take up the motion of the air so readily as the air itself. They will allow the air to move backwards and forwards past them, and so cause friction and diminish the effect.'

Further on it will be proved, both by observation and experiment, that fog exercises no such power upon sound as that above ascribed to it.

§ 3. *Instruments and Observations.*

The foregoing extracts and references sufficiently indicate the uncertain state of the question when, on the 19th of May, 1872, this inquiry began. The South Foreland, near Dover, was chosen as the signal-station, steam-power

having been already established there to work two powerful magneto-electric lights. The observations for the most part were made afloat, one of the yachts of the Trinity Corporation being usually employed for this purpose. Two stations had been established, the one at the top, the other at the bottom of the South-Foreland Cliff; and at each of them trumpets and air- and steam-whistles of great size were mounted. The whistles first employed were of English manufacture; but intelligence having been received regarding a large United States whistle, and also a Canadian whistle, of great reputed power, the Elder Brethren had them subsequently added to the list.

On the 8th of October another instrument, which has played a specially important part in these observations, was introduced. During my visit to America I accompanied General Woodruff to Sandy Hook, with the express intention of observing the performance of a steam-syren, which, under the auspices of Professor Joseph Henry, had been introduced into the lighthouse system of the United States. I carried home with me a somewhat vivid remembrance of the mechanical effect of the sound upon my ears and body generally. Hence my desire to see the syren tried at the South Foreland. The formal expression of this desire was anticipated by the Elder Brethren, while their wishes were in turn anticipated by the courteous kindness of the Lighthouse Board at Washington. Informed by Major Elliot, of the United States Army, that our experiments had begun, the Board forwarded to the Corporation, for trial, the powerful instrument which was mounted at the South Foreland.

In the steam-syren patented by Mr. Brown, of New York, a fixed disc and a rotating disc are employed as in the ordinary syren, radial slits being cut in both discs instead of circular apertures. One disc is fixed vertically across the throat of a conical trumpet $16\frac{1}{2}$ feet long, 5

inches in diameter where the disc crosses it, and gradually opening out till at the other extremity it reaches a diameter of 2 feet 3 inches. Behind the fixed disc is the rotating one, which is driven by separate mechanism. The trumpet is connected with a boiler. In our experiments steam of 70 lbs. pressure was for the most part employed. Just as in the ordinary syren, when the radial slits of the two discs coincide, and then only, a strong puff of steam escapes. Sound-waves of great intensity are thus sent through the air, the pitch of the note produced depending on the velocity of rotation.

To the syren, trumpets, and whistles were added three guns—an 18-pounder, a 5½-inch howitzer, and a 13-inch mortar. In our summer experiments all three were fired; but the howitzer having shown itself superior to the other guns, it was chosen, in our autumn experiments, as not only a fair but a favourable representative of this form of signal. The charges fired were for the most part those now employed at Holyhead, Lundy Island, and the Kish light-vessel—namely, 3 lbs. of powder. Gongs and bells were not included in this inquiry, because previous observations had clearly proved their inferiority to the trumpets and whistles.

A general knowledge of the instruments employed is thus imparted to the reader; while the Map, fig. 155, will furnish him with information as to the position of most of the localities referred to in the paper.

On May 19 the instruments tested were:—

On the top of the cliff:

(*a*) Two brass trumpets or horns, 11 feet 2 inches long, 2 inches in diameter at the mouthpiece, and opening out at the other end to a diameter of 22½ inches. They were provided with vibrating steel reeds 9 inches long, 2 inches wide, and ¼ inch thick, and were sounded by air of 18 lbs. pressure.

Fig. 155.

C.&D.R.
Wood
Walmer
Walmer Castle
Ringwold
KINGSDOWN
SOUTH GODWIN
Ewell
St Radigands Abbey
St Margarets
Charlton
DOVER
Electric Lights
Cornhill C.G.
St Margaret Bay
Sth Sand Head Light Vessel
HYTHE
C.G.
Eastware B.
LYDDEN SPOUT
Shakespeare Cliff
ADMIRALTY PIER
Sth Foreland
Folkestone
Telegraph Cable Dover to Calais
To La Panne
VARNE BUOY

(*b*) A whistle, shaped like that of a locomotive, 6 inches in diameter, also sounded by air of 18 lbs. pressure.

(*c*) A steam-whistle, 12 inches in diameter, attached to a boiler, and sounded by steam of 64 lbs. pressure.

At the bottom of the cliff:

(*d*) Two trumpets or horns, of the same size and arrangement as those above, and sounded by air of the same pressure.

(*e*) A 6-inch air-whistle, similar to the one above, and sounded by the same means.

The upper instruments were 235 feet above high-water mark, the lower ones 40 feet. A vertical distance of 195 feet, therefore, separated the instruments. A shaft, provided with a series of twelve ladders, led from the one to the other.

The trumpets were constructed by that able mechanician, Mr. HOLMES, who had them throughout under his personal superintendence. They were mounted vertically on the reservoir of compressed air; but within about 2 feet of their extremities they were bent at a right angle, so as to present their mouths to the sea. The aim of their constructor was to distribute the sound equably over an arc of 180°. To effect this, he placed the horizontal parts of the axes of the horns at right angles to each other, the one pointing S.W. by S., and the other S.E. by E., each horn being supposed to cover an arc of 90°.

The 12-inch steam-whistle was constructed by Mr. BAILY, of Manchester.

Our first experiments with these instruments were a preliminary discipline rather than an organised effort at discovery. On May 19 we steamed round the Foreland and out to sea in the axes of the horns. The maximum distance reached by the sound was about three and a half miles.[1] The wind, however, was high and the sea rough,

[1] In all cases nautical miles are meant.

so that local noises interfered to some extent with our appreciation of the sound.

Mariners express the strength of the wind by a series of numbers extending from 0 = calm to 12 = a hurricane, a little practice, in common, producing a remarkable unanimity between different observers as regards the force of the wind. Its force on May 19 was 6, and midway between the axes of the two trumpets it blew at right angles to the direction of the sound.

The same instruments on the 20th of May covered a greater range; but not much greater, though the disturbance due to local noises was absent. At 4 miles distance in the axes of the horns they were barely heard, the air at the time being calm, the sea smooth, and all other circumstances exactly those which have been hitherto regarded as most favourable to the transmission of sound. We crept a little further away, and by stretched attention managed to hear at intervals, at a distance of 6 miles, the faintest hum of the horns. A little further out we again halted; but though local noises were absent, and though we listened intently, we heard nothing.

This position, clearly beyond the range of whistles and trumpets, was expressly chosen with the view of making what might be considered a decisive comparative experiment between horns and guns as instruments for fog-signalling. The distinct report of the 12 o'clock gun fired at Dover on the 19th suggested this comparison, and through the prompt courtesy of General Sir A. Horsford we were enabled to carry it out. At 12.30 precisely the puff of an 18-pounder, with a 3-lb. charge, was seen at Dover Castle, which was about a mile further off than the South Foreland. Thirty-six seconds afterwards the loud report of the gun was heard, its complete superiority over the trumpets being thus, to all appearance, demonstrated. We clinched this observation by steaming out to a distance

of $8\frac{1}{2}$ miles, where the report of a second gun was well heard by all of us. At a distance of 10 miles the report of a third gun was heard by some of us, and at 9·7 miles the report of a fourth gun was heard by us all.

The result seemed perfectly decisive. Applying the law of inverse squares, the sound of the gun at a distance of 6 miles from the Foreland must have had about three times the intensity of the sound of the trumpets. It would hardly have been rash under the circumstances to have reported without qualification the superiority of the gun as a fog-signal. No single experiment is, to my knowledge, on record to prove that a sound once predominant would not be always predominant, or that the atmosphere on different days would show preferences to different sounds. On many subsequent occasions, however, the sound of the horn proved distinctly superior to that of the gun. This *selective* power of the atmosphere revealed itself more strikingly in our autumn experiments than in our summer ones; and it was sometimes illustrated within a few hours of the same day: of two sounds A and B, for example, A would have the greatest range at 10 A.M. and B the greatest range at 2 p.m.

In the experiments on the 19th and 20th of May the superiority of the trumpets over the whistles was decided; and indeed, with few exceptions, this superiority was maintained throughout the inquiry. But there were exceptions. On June 2, for example, the sound of the whistles rose in several instances to full equality with, and on rare occasions subsequently even surpassed, that of the horns. The sounds were varied from day to day. On the date last mentioned a single horn was sounded, two were sounded, and three were sounded together; but the utmost range of the loudest sound, even with the paddles stopped, did not exceed six miles. With the view of concen-

trating their power, the axes of the horns were, on June 2, pointed in the same direction, and, unless stated to the contrary, this in all subsequent experiments was the case.

On the 3rd of June the three guns already referred to were permanently mounted at the South Foreland. They were well served by gunners from Dover Castle.

At a certain hour of the same day, dense clouds quite covered the firmament, some of them particularly black and threatening, but a marked advance was observed in the transmissive power of the air. At a distance of 6 miles the horn-sounds were not quite quenched by the paddle-noises; at 8 miles the whistles were heard, and the horns better heard; while at 9 miles, with the paddles stopped, the horn-sounds alone were fairly audible. A remarkable and instructive phenomenon was now observed. Over us rapidly passed a torrential shower of rain, which has been hitherto regarded as a potent damper of sound. I could, however, notice no subsidence of intensity as the shower passed. It is even probable that, had my mind been free from bias, I should have noticed an augmentation of the sound, such as occurred with the greatest distinctness on various subsequent occasions during violent rain.

The influence of 'beats' was tried on June 3, by throwing the horns slightly out of unison; but though the beats rendered the sound characteristic, they did not seem to augment the range. At a distance from the station curious fluctuations of intensity were noticed. Not only did the different blasts vary in strength, but sudden swellings and fallings-off, even of the same blast, were observed. This was not due to any variation on the part of the instruments, but purely to the changes of the medium traversed by the sound.

During the inquiry various shiftings of the horns and reeds were resorted to, with a view of bringing out their

maximum power. The range of our best horns on June 10 was $8\frac{3}{4}$ miles. The guns at this distance were very feeble. That the loudness of the sound depends on the shape of the gun was proved by the fact that thus far the howitzer, with a 3-lb. charge, proved more effective than the other guns. In the axis of the horns the sound manifests its greatest strength, falling sensibly off as the angular distance from the axis is augmented.[1] Now, the whistles have no such axes, but send their sound-waves with equal strength in all directions. Hence, as the horns pointed seaward, near the line joining the Foreland and the South Sand Head light-vessel on the one hand, and that joining the Foreland and the Admiralty Pier on the other, the whistles were sometimes more than a match for the horns.

§ 4. *Influence of Sound-Shadow.*

On the 19th of May we noticed a phenomenon of grave import in connection with the establishment of fog-signals. I refer to the rapid fall of intensity on both sides of the signal-station at the South Foreland. We had halted between the station and the South Sand Head light-ship, at a distance of $2\frac{1}{2}$ miles from the station. The trumpets and whistles were sounded, but they were quite unheard. We moved nearer; but even at a mile distance, with the instruments plainly in view, their sound failed to reach us. A light wind, however, was here opposed to the sound. Abreast of the signal-station the trumpets were very powerful; but on approaching the line joining the Foreland to the end of the Admiralty Pier the sound fell rapidly, though in this case the wind was favourable to the sound. Some other cause than the

[1] When, subsequently, we compared the sound in the prolonged axis of a gun with the intensity at right angles to the axis, the difference, though sensible, was very small. This agrees with previous observations. The sound-waves are practically of the same intensity all round the gun.

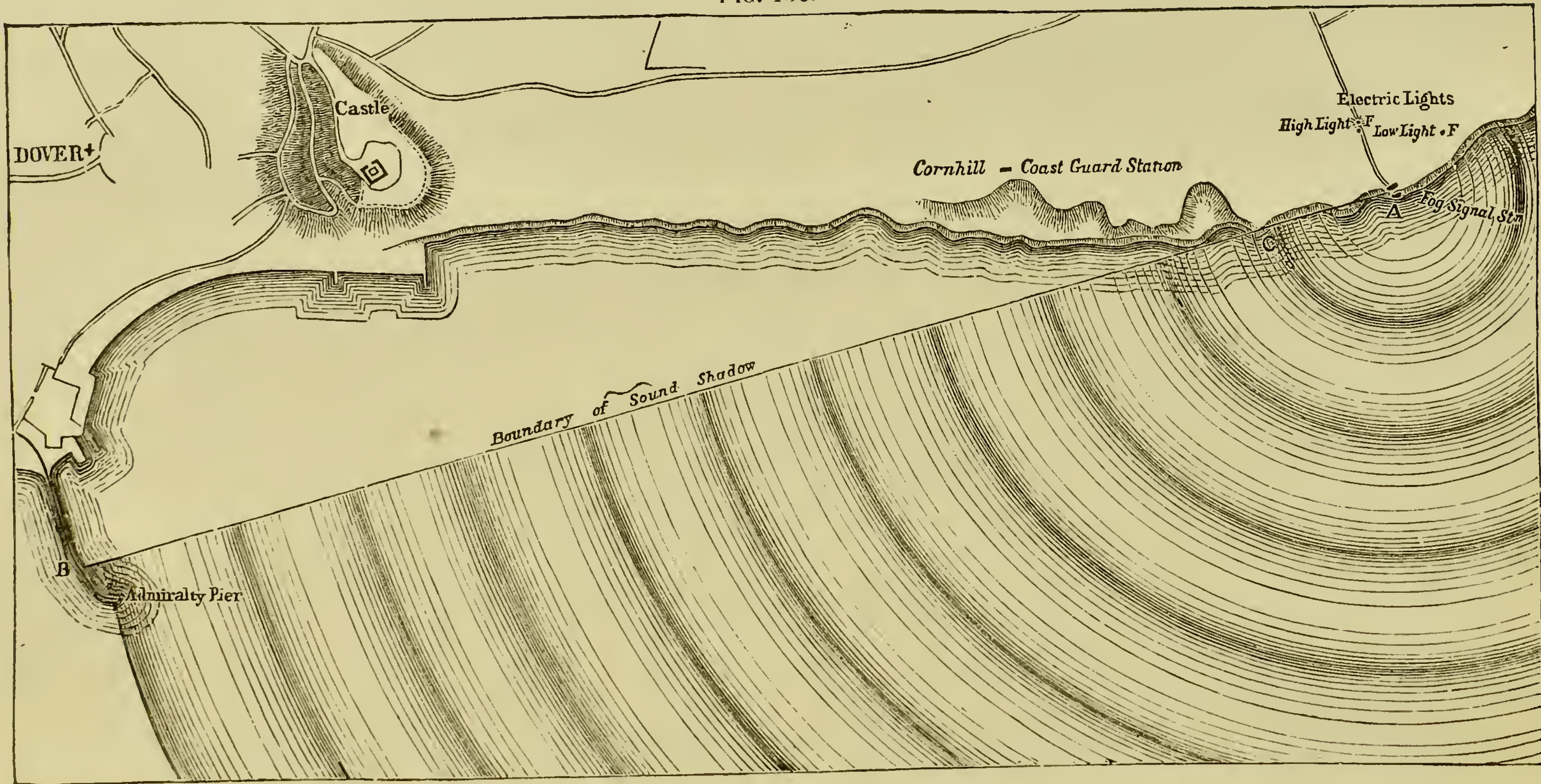
wind must, therefore, be invoked to account for the phenomenon.

On the 10th of June the same effect was very strikingly manifested. After our day's work we steamed past the Foreland and towards the end of the Pier. At the distance of a mile from the Foreland the sounds fell with such rapidity that I thought something must have occurred to the whistles and horns. Happily the guns were there to test this surmise. At 2 miles distance we signalled for them. With a 3-lb. charge, though their puffs were clearly seen, not one of them was heard; with a 6-lb. charge the 18-pounder was barely heard, the howitzer was slightly better heard, while the mortar was quite unheard. No peculiarities of the horns or whistles could therefore account for the phenomenon.

On the 11th of June the effect was equally pronounced. On the line joining the Foreland and the end of the Admiralty Pier, and at $\frac{3}{4}$ of a mile from the station, the sound rapidly sank in power, and soon afterwards became inaudible. At $1\frac{1}{4}$ mile distance we signalled for the guns; the report in each case was a low indistinct thud. A necessary requirement in fog-signals is stated to be that they should, under all circumstances, be heard to a distance of 4 miles. Now, the gun was undoubtedly the signal of greatest range when this inquiry began, and here we find that conditions may exist which render even the gun ineffectual at less than half the distance deemed essential.

The Map, fig. 156, which consists of a portion of fig. 155 enlarged, will help us to an explanation of these observations. Near the fog-signal station a projecting chalk cliff at C received the impact of the sonorous waves and dispersed them by reflection. The whole sea space between the line A B and the cliffs under Dover Castle was in the sound-shadow. Within this line the instruments could not be seen, without it they could; and

Fig. 156.



DOVER
Castle
Cornhill
Coast Guard Station
Electric Lights
High Light F
Low Light F
A
Fog Signal Stn
C
Boundary of Sound Shadow
B
Admiralty Pier

we have to account for the fact that the enfeeblement of the sound occurred not only inside but immediately outside the boundary, and while the instruments were in sight. A sudden subsidence of the sound was always observed on crossing the boundary A B towards the shore, and a correspondingly sudden augmentation on crossing it towards the sea; but the stoppage of the sound on entering the shadow was by no means total. The whole of the shaded space was filled with sound of enfeebled intensity, produced in great part by the divergence into the shadow of the waves which abutted against the boundary. Through this divergence the direct waves had their intensity lowered, the portions nearest to the shadow suffering most. (On the Map the condensations and rarefactions of the direct waves are shown by circular lines of varying closeness.) Here, then, we have one cause of the decay of the sound in the neighbourhood of the acoustic shadow. Another cause may be the interference[1] of the direct waves with those reflected from C and from other portions of the cliff. The remarks here, applied to the sound-shadow west of the Foreland, are also applicable to that upon the other side.

On June 25 a gradual improvement in the transmissive power of the air was observed from morning to evening; but at the last the maximum range was only moderate. The fluctuations in the strength of the sound were remarkable, sometimes sinking to inaudibility and then rising to loudness. A similar effect, due to a similar cause, is often noticed with church bells. The acoustic transparency of the air was still further augmented on the 26th: at a distance of $9\frac{1}{4}$ miles from the station the whistles and horns were plainly heard against a wind with a force of 4; while on the 25th, with a favouring wind,

[1] The principles of interference are set forth in Lecture VIII.

the maximum range was only $6\frac{1}{2}$ miles. *Plainly, therefore, something else than the wind must be influential in determining the range of the sound.*

On Tuesday, July 1, observations were made on the decay of the sound at various angular distances from the axis of the horn. As might be expected the sound in the axis was loudest, the decay being gradual on both sides. The day was acoustically clear. At a distance of 10 miles the horn yielded a plain sound, while the American whistle seemed to surpass the horn. Dense haze at this time quite hid the Foreland. At $10\frac{1}{2}$ miles occasional blasts of the horn came to us, but after a time all sounds ceased to be audible ; it seemed as if the air, after having been exceedingly transparent, had become gradually opaque to the sound.

At 4.45 P.M. we took the master of the Varne light-ship on board the *Irene*. He and his company had heard the sound at intervals during the day, although he was dead to windward and distant $12\frac{3}{4}$ miles from the source of sound.

Here a word of reflection on our observations may be fitly introduced. It is, as already shown, an opinion generally entertained that the waves of sound are reflected at the limiting surfaces of the minute particles which constitute haze and fog, the alleged waste of sound in fog being thus explained. If, however, this be an efficient practical cause of the stoppage of sound, and if clear calm air be, as alleged, the best vehicle, it would be impossible to understand how to-day, in a thick haze, the sound reached a distance of $12\frac{3}{4}$ miles, while on May 20, in a calm and hazeless atmosphere, the maximum range was only from 5 to 6 miles. Such facts foreshadow a revolution in our notions regarding the action of haze and fogs upon sound, and the revolution will be complete.

An interval of 12 hours sufficed to change in a sur-

prising degree the acoustic transparency of the air. On the 1st of July the sound had a range of nearly 13 miles; on the 2nd the range did not exceed 4 miles.

§ 5. *Contradictory Results.*

Thus far the investigation proceeded with hardly a gleam of a principle to connect the inconstant results. The distance reached by the sound on the 19th of May was 3½ miles; on the 20th it was 5½ miles; on the 2nd of June 6 miles; on the 3rd more than 9 miles; on the 10th it was also 9 miles; on the 25th it fell to 6½ miles; on the 26th it rose again to more than 9¼ miles; on the 1st of July, as we have just seen, it reached 12¾, whereas on the 2nd the range shrank to 4 miles. None of the meteorological agents observed at the time could be singled out as the cause of these fluctuations. The wind exerts an acknowledged power over sound, but it could not account for all these phenomena. On the 25th of June, for example, when the range was only 6½ miles, the wind was favourable; on the 26th, when the range exceeded 9¼ miles, it was opposed to the sound. Nor could the varying optical clearness of the atmosphere be invoked as an explanation; for on July 1, when the range was 12¾ miles, a thick haze hid the white cliffs of the Foreland, while on many other days, when the acoustic range was not half so great, the atmosphere was optically clear. Up to July 3 all remained enigmatical; but on this date observations were made which seemed to me to displace surmise and perplexity by the clearer light of physical demonstration.

§ 6. *Proposed Solution of Contradictions.*

On July 3 we first steamed to a point 2·9 miles S.W. by W. of the signal station. No sounds, not even the

guns, were heard at this distance. At 2 miles they were equally inaudible. But this being a position at which the sounds, though strong in the axis of the horn, invariably subsided, we steamed to the exact bearing from which our observations had been made on July 1. At 2.15 P.M., and at a distance of $3\frac{3}{4}$ miles from the station, with calm clear air and a smooth sea, the horns and whistle (American) were sounded, but they were inaudible. Surprised at this result, we signalled for the guns. They were all fired, but, though the smoke seemed close at hand, no sound whatever reached us. On July 1, in this bearing, the observed range of both horns and guns was $10\frac{1}{2}$ miles, while on the bearing of the Varne light-vessel it was nearly 13 miles. We steamed in to 3 miles, paused, and listened with all attention; but neither horn nor whistle was heard. The guns were again signalled for; five of them were fired in succession, but not one of them was heard. We steamed on in the same bearing to 2 miles, and had the guns fired point blank at us. The howitzer and the mortar, with 3 lb.-charges, yielded a feeble thud, while the 18-pounder was wholly unheard. Applying the law of inverse squares, it follows that, with the air and sea, according to accepted notions, in a far worse condition, the sound at 2 miles' distance on July 1 must have had more than forty times the intensity which it possessed at the same distance at 3 P.M. on the 3rd.

'On smooth water,' says Sir John Herschel, 'sound is propagated with remarkable clearness and strength.' Here was the condition; still with the Foreland so close to us, the sea so smooth, and the air so transparent, it was difficult to realise that the guns had been fired or the trumpets blown at all. What could be the reason?

Sulphur in homogeneous crystals is exceedingly transparent to radiant heat, whereas the ordinary brimstone of commerce is highly impervious to it—the reason being

that the brimstone does not possess the molecular continuity of the crystal, but is a mere aggregate of minute grains not in perfect optical contact with each other. Where this is the case a portion of the heat is always reflected on entering and on quitting a grain; hence, when the grains are minute and numerous, this reflection is so often repeated that the heat is entirely wasted before it can plunge to any depth into the substance. The same remark applies to snow, foam, clouds, and common salt, indeed to all transparent substances in powder; they are all impervious to light, not through the absorption of the light, but through repeated internal reflection.

Humboldt, in his observations at the Falls of the Orinoco, is known to have applied these principles to sound. He found the noise of the falls far louder by night than by day, though in that region the night is far noisier than the day. The plain between him and the falls consisted of spaces of grass and rock intermingled. In the heat of the day he found the temperature of the rock to be considerably higher than that of the grass. Over every heated rock, he concluded, rose a column of air rarefied by the heat; its place being supplied by the descent of heavier air. He ascribed the deadening of the sound to the reflections which occurred at the limiting surfaces of the rarer and denser air. This philosophical explanation made it generally known that a non-homogeneous atmosphere is unfavourable to the transmission of sound.

But what on July 3, not with a variously heated plain, but with a calm sea as a basis for the atmosphere, could so destroy its homogeneity as to enable it to quench in so short a distance so vast a body of sound? My course of thought at the time was thus determined:—As I stood upon the deck of the *Irene* pondering the question, I became conscious of the exceeding power of the sun beating

against my back and heating the objects near me. Beams of equal power were falling on the sea, and must have produced copious evaporation. That the vapour generated should so rise and mingle with the air as to form an absolutely homogeneous medium was in the highest degree improbable. It would be sure, I thought, to rise in invisible streams, breaking through the superincumbent air now at one point, now at another, thus rendering the air *flocculent* with wreaths and striæ. At the limiting surfaces of these we should have the conditions necessary to the production of partial echoes and the consequent waste of sound. Air-currents of different temperatures, as far as they existed, would also contribute to the effect.

The conditions necessary for the testing of this explanation immediately set in. At 3.15 P.M. a solitary cloud threw itself athwart the sun, and shaded the entire space between us and the South Foreland. The heating of the water and the production of vapour and air-currents were checked by the interposition of this screen; hence the probability of suddenly improved transmission. To test this inference the steamer was immediately turned and urged back to our last position of inaudibility. The sounds, as I expected, were distinctly though faintly heard. This was at 3 miles' distance. At $3\frac{3}{4}$ miles the guns were fired, both point blank and elevated. The faintest pop was all that we heard; but we did hear a pop, whereas we had previously heard nothing, either here or three-quarters of a mile nearer. We steamed out to $4\frac{1}{4}$ miles, where the sounds were for a moment faintly heard; but they fell away as we waited; and though the greatest quietness reigned on board, and though the sea was without a ripple, we could hear nothing. We could plainly see the steam-puffs which announced the beginning and the end of a series of trumpet-blasts, but the blasts themselves were quite inaudible.

It was now 4 P.M., and my intention at first was to halt at this distance, which was beyond the sound-range, but not far beyond it, and see whether the lowering of the sun would not restore the power of the atmosphere to transmit the sound. But after waiting a little the anchoring of a boat was suggested, so as to liberate the steamer for other work; and though loth to lose the anticipated revival of the sounds myself, I agreed to this arrangement. Two men were placed in the boat and requested to give all attention, so as to hear the sound if possible. With perfect stillness around them they heard nothing. They were then instructed to hoist a signal if they should hear the sounds, and to keep it hoisted as long as the sounds continued.

At 4.45 we quitted them and steamed towards the South Sand Head light-ship. Precisely 15 minutes after we had separated from them the flag was hoisted; the sound having at length succeeded in piercing the body of air between the boat and the shore. On our return, of course we heard the sounds ourselves.

We pushed the test further by steaming further out. At $5\frac{3}{4}$ miles we halted and heard the sounds; at 6 miles we heard them distinctly, but so feebly that we thought we had reached the limit of the sound-range; but while we waited the sounds rose in power. We steamed to the Varne buoy, which is $7\frac{3}{4}$ miles from the signal-station, and heard the sounds there better than at 6 miles' distance. We continued our course outwards to 10 miles, halted there for a brief interval, but heard nothing.

Steaming, however, on to the Varne light-ship, which is situated at the other end of the Varne shoal, we hailed the master, and were informed by him that up to 5 P.M. nothing had been heard, but that at that hour the sounds began to be audible. He described one of them as 'very gross, resembling the bellowing of a bull,' which very

accurately characterises the sound of the large American steam-whistle. At the Varne light-ship, therefore, the sounds had been heard towards the close of the day, though it is $12\frac{3}{4}$ miles from the signal-station. I think it probable that, at a point 2 miles from the Foreland, the sound at 5 P.M. possessed fifty times the intensity which it possessed at 2 P.M. To such undreamt-of fluctuations is the atmosphere liable. On our return to Dover Bay, at 10 P.M., we heard the sounds, not only distinct but loud, where nothing could be heard in the morning.

§ 7. *Echoes from Invisible Acoustic Clouds.*

But both the argument and the phenomena have a complementary side, which we have now to consider. A stratum of air less than 3 miles thick on a calm day has been thus proved competent to stifle both the cannonade and the horn-sounds employed at the South Foreland; while, according to the foregoing explanation, this result was due to the reflection of the sound from invisible *acoustic clouds* which filled the atmosphere on a day of perfect *optical* transparency. But, granting this, it is incredible that so great a body of sound could utterly disappear in so short a distance without rendering some account of itself. Supposing, then, instead of placing ourselves behind the acoustic cloud, we were to place ourselves in front of it, might we not, in accordance with the law of conservation, expect to receive, by reflection, the sound which had failed to reach us by transmission? The case would then be strictly analogous to the reflection of light from an ordinary cloud to an observer between it and the sun.

My first care in the early part of the day in question was to assure myself that our inability to hear the sound did not arise from any derangement of the instruments on shore. Accompanied by Mr. Price Edwards, at 1 P.M. I

was rowed to the shore, and landed at the base of the South Foreland Cliff. The body of air which had already shown such extraordinary power to intercept the sound, and which manifested this power still more impressively later in the day, was now in front of us. On it the sonorous waves impinged, and from it they were sent back with astonishing intensity. The instruments, hidden from view, were on the summit of a cliff 235 feet above us, the sea was smooth and clear of ships, the atmosphere was without a cloud, and there was no object in sight which could possibly produce the observed effect. From the perfectly transparent air the echoes came, at first with a strength apparently little less than that of the direct sound, and then dying away. A remark made by my talented companion in his note-book at the time shows how the phenomenon affected him:—'Beyond saying that the echoes seemed to come from the expanse of ocean, it did not appear possible to indicate any more definite point of reflection.' Indeed, no such point was to be seen; the echoes reached us, as if by magic, from the invisible acoustic clouds with which the optically transparent atmosphere was filled. The existence of such clouds in all weathers, whether optically cloudy or serene, is one of the most important points established by this inquiry.

Here, in my opinion, we have the key to many of the mysteries and discrepancies of evidence which beset this question. The foregoing observations show that there is no need to doubt either the veracity or the ability of the conflicting witnesses, for the variations of the atmosphere are more than sufficient to account for theirs. The mistake indeed hitherto has been, not in reporting incorrectly, but in neglecting the monotonous operation of repeating the observations during a sufficient time. I shall have occasion to remark subsequently on the mischief likely to

arise from giving instructions to mariners founded on observations of this incomplete character.

It required, however, long pondering and repeated observation before this conclusion took firm root in my mind; for it was opposed to the results of great observers, and to the statements of celebrated writers. While cloud-echoes have been accepted as demonstrated by observation, it has been hitherto held as established that audible echoes never occur in optically clear air. We owe this opinion to the otherwise admirable report of Arago on the experiments made at Mont Montlhéry and Villejuif in 1822 to determine the velocity of sound.[1] Arago's account of the phenomenon observed by him and his colleagues is as follows:—'Before ending this note we will only add that the shots fired at Montlhéry were accompanied by a rumbling like that of thunder, which lasted from 20 to 25 seconds. Nothing of this kind occurred at Villejuif. Once we heard two distinct reports, a second apart, of the Montlhéry cannon. In two other cases the report of the same gun was followed by a prolonged rumbling. These phenomena never occurred without clouds. Under a clear sky the sounds were single and instantaneous. May we not therefore conclude that the multiple reports of the Montlhéry gun heard at Villejuif were echoes from

[1] Sir John Herschel gives the following account of Arago's observation:—'The rolling of thunder has been attributed to echoes among the clouds; and if it is considered that a cloud is a collection of particles of water, however minute, in a liquid state, and therefore each individually capable of reflecting sound, there is no reason why very loud sounds should not be reverberated confusedly (like bright lights) from a cloud. And that such is the case has been ascertained by direct observation on the sound of cannon. Messrs. Arago, Matthieu, and Prony, in their experiments on the velocity of sound, observed that under a perfectly clear sky the explosions of their guns were always single and sharp; whereas when the sky was overcast, and even when a cloud came in sight over any considerable part of the horizon, they were frequently accompanied by a long-continued roll like thunder.'—*Essay on Sound*, par. 38. The distant clouds would imply a long interval between sound and echo, but nothing of the kind is reported.

the clouds, and may we not accept this fact as favourable to the explanation given by certain physicists of the rolling of thunder?'

My reply to this question would be a frank negative. For, hundreds of cannon-shots were fired at the South Foreland, many of them when the heavens were completely free from clouds, and never in a single case was a *roulement* similar to that noticed by Arago absent. It followed, moreover, so hot upon the direct sound as to present hardly a sensible breach of continuity between the sound and the echo. This could not be the case if the clouds were its origin. A reflecting cloud, at the distance of a mile, would leave a silent interval of nearly 10 seconds between sound and echo; and had such an interval been observed, it could hardly have escaped record by the French philosophers stationed there; but they have not recorded it.

I think both the alleged fact and the inference from it need re-consideration. For our observations prove to demonstration that air of perfect visual transparency is competent to produce echoes of great intensity and long duration. The subject is worthy of additional illustration. On the 8th of October, as already stated, the syren was established at the South Foreland. I visited the station on that day, and listened to its echoes. They were far more powerful than those of the horn. Like the others they were perfectly continuous, and faded, as it into distance, gradually away. The direct sound seemed rendered complex and multitudinous by its echoes, which resembled a band of trumpeters first responding close at hand, and then retreating rapidly towards the coast of France. The syren-echoes on that day had 11 seconds, those of the horn 8 seconds' duration.

In the case of the syren, moreover, the reinforcement of the direct sound by its echo was distinct. About a

second after the commencement of the syren-blast the echo struck in as a new sound. This first echo, therefore, must have been flung back by a body of air not more than 600 or 700 feet in thickness. The few detached clouds visible at the time were many miles away, and could clearly have had nothing to do with the effect.

On the 10th of October I was again at the Foreland listening to the echoes, with results similar to those just described. On the 15th I had an opportunity of remarking something new concerning them at Dungeness, where a horn similar to, though not so powerful as, those at the South Foreland, had been mounted. It rotated automatically through an arc of 210°, halting at four different points on the arc and emitting a blast of 6 seconds' duration, these blasts being separated from each other by intervals of silence of 20 seconds.

The new point observed was this: as the horn rotated the echoes were always returned along the line in which the axis of the horn pointed. Standing either behind or in front of the lighthouse tower, or closing the eyes so as to exclude all knowledge of the position of the horn, the direction of its axis when it sounded could always be inferred from the direction in which the aerial echoes reached the shore. Not only, therefore, is knowledge of *direction* given by a sound, but it may also be given by the aerial echoes of the sound.

On the 17th of October, at about 5 P.M., the air being perfectly free from clouds, we rowed towards the Foreland, landed, and passed over the seaweed to the base of the cliff. As I reached the base the position of the *Galatea* was such that an echo of astonishing intensity was sent back from her side; it came as if from an independent source of sound established on board the steamer. This also ceased suddenly, leaving the aerial echoes to die gradually into silence.

At the base of the cliff a series of concurrent observations made the duration of the aerial syren-echoes from 13 to 14 seconds.

Lying on the shingle under a projecting roof of chalk, the somewhat enfeebled diffracted sound reached me, and I was able to hear with great distinctness, about a second after the starting of the syren-blast, the echoes striking in and reinforcing the direct sound. The first rush of echoed sound was very powerful, and it came, as usual, from a stratum of air 600 or 700 feet in thickness. On again testing the duration of the echoes, it was found to be from 14 to 15 seconds. The perfect clearness of the afternoon caused me to choose it for the examination of the echoes. It is worth remarking that this was our day of longest echoes, and it was also our day of greatest acoustic transparency, this association suggesting that the duration of the echo is a measure of the atmospheric *depths* from which it comes. On no day, it is to be remembered, was the atmosphere free from invisible acoustic clouds; and on this day, when their presence did not prevent the direct sound from reaching to a distance of 16 nautical miles, they were able to send us echoes of 15 seconds' duration.

On various occasions, when we were fully three miles from the shore, the Foreland bearing north, we have had the distinct echoes of the syren sent back to us from the cloudless *southern* air.

To sum up this question of aerial echoes. The syren sounded three blasts a minute, each of 5 seconds' duration. From the number of days and the number of hours per day during which the instrument was in action we can infer the number of blasts. They reached nearly twenty thousand. The blasts of the horns exceeded this number, while hundreds of shots were fired from the guns. Whatever might be the state of the weather, cloudy or serene,

stormy or calm, the aerial echoes, though varying in strength and duration from day to day, were never absent; and on many days, 'under a perfectly clear sky,' they reached, in the case of the syren, an astonishing intensity. It is doubtless to these air-echoes, and not to cloud-echoes, that the rolling of thunder is in great part to be ascribed.

§ 8. *Experimental Demonstration of the Reflection of Sound from Gases.*

Thus far we have dealt in inference merely, for the interception of sound through aerial reflection has never been experimentally demonstrated; and, indeed, according to Arago's observation, which has hitherto held undisputed possession of the scientific field, it does not sensibly exist. But the strength of science consists in verification, and I was anxious to submit the question of aerial reflection to an experimental test. The knowledge gained in the last lecture enables us to apply such a test; but as in most similar cases, it was not the simplest combinations that were first adopted. Two gases of different densities were to be chosen, and I chose carbonic acid and coal-gas. With the aid of my skilful assistant, a tunnel was formed, across which five-and-twenty layers of carbonic acid were permitted to fall, and five-and-twenty alternate layers of coal-gas to rise. Sound was sent through this tunnel, making fifty passages from medium to medium in its course. These, I thought, would waste in aerial echoes a sensible portion of the sound.

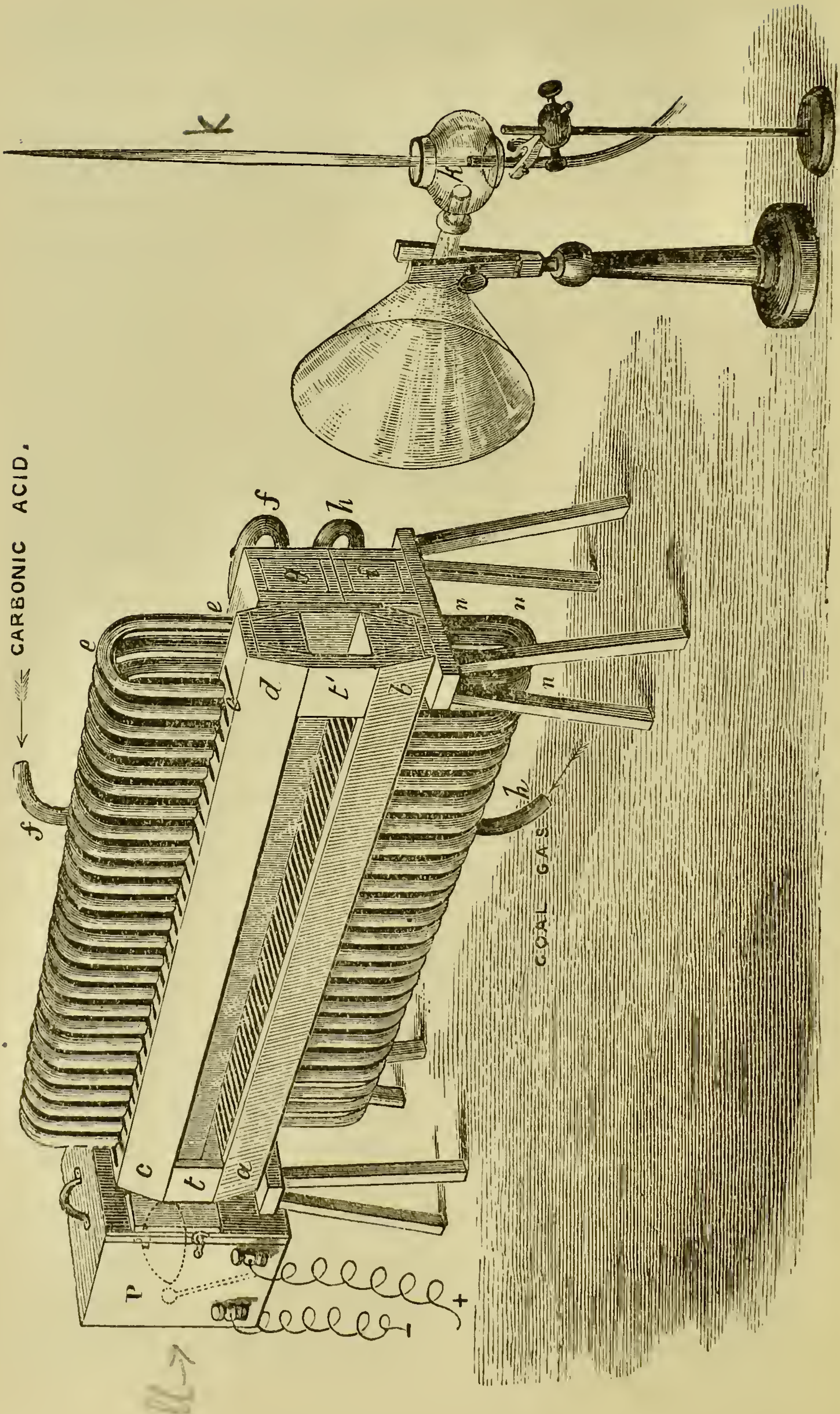
To indicate this waste an objective test was found in one of the sensitive flames described in the last chapter. Acquainted with it, we are prepared to understand a drawing and description of the apparatus first employed by me in the demonstration of aerial reflection. The

following account of the apparatus was given by a writer in 'Nature,' Feb. 5, 1874:—

'A tunnel *t t′* (fig. 157), 2 in. square, 4 ft. 8 in. long, open at both ends, and having a glass front, runs through the box, *a b c d*. The spaces above and below are divided into cells opening into the tunnel by transverse orifices exactly corresponding vertically. Each alternate cell of the upper series—the 1st, 3rd, 5th. &c.—communicates by a bent tube (*e e e*) with a common upper reservoir (*g*), its counterpart cell in the lower series having a free outlet into the air. In like manner the 2nd, 4th, 6th, &c., of the lower series of cells are connected by bent tubes (*n n n*) with the lower reservoir (*i*), each having its direct passage into the air through the cell immediately above it. The gas distributors (*g* and *i*) are filled from both ends at the same time, the upper with carbonic acid-gas, the lower with coal-gas, by branches from their respective supply pipes (*f* and *h*). A well-padded box (P) open to the end of the tunnel forms a little cavern, whence the sound-waves are sent forth by an electric bell (dotted in the figure). A few feet from the other end of the tunnel, and in a direct line with it, is a sensitive flame (*k*), provided with a funnel as sound-collector, and guarded from chance currents by a shade.

'The bell was set ringing. The flame, with quick response to each blow of the hammer, emitted a sort of musical roar, shortening and lengthening as the successive sound-pulses reached it. The gases were then admitted. Twenty-five flat jets of coal-gas ascended from the tubes below, and twenty-five cascades of carbonic acid fell from the tubes above. That which had been a homogeneous medium had now fifty limiting surfaces, from each of which a portion of the sound was thrown back. In a few moments these successive reflections became so effective that no sound having sufficient power to affect

FIG. 107.



the flame could pierce the clear, optically-transparent, but acoustically-opaque atmosphere in the tunnel. So long as the gases continued to flow the flame remained perfectly tranquil. When the supply was cut off, the gases rapidly diffused into the air. The atmosphere of the tunnel became again homogeneous, and therefore acoustically transparent, and the flame responded to each sound-pulse as before.'

Not only do gases of different densities act thus upon sound, but atmospheric air in layers of different temperatures does the same. Across a tunnel resembling *t t'* (fig. 157), sixty-six platinum wires were stretched, all of them being in metallic connection. The bell, in its padded box, was placed at one end of the tunnel, and the sensitive flame *k*, near its flaring point, at the other. When the bell rang the flame flared. A current from a strong voltaic battery, being sent through the platinum wires, they became heated: layers of warm air rose from them through the tunnel, and immediately the agitation of the flame was stilled. On stopping the current, the agitation recommenced. In this experiment the platinum wires had not reached a red heat. Employing half the number and the same battery, they were raised to a red heat, the action in this case upon the sound-waves being also energetic. Employing one-third of the number of wires, and the same strength of battery, the wires were raised to a white heat. Here also the flame was immediately rendered tranquil by the stoppage of the sound.

§ 9. *Reflection from Vapours.*

But not only do gases of different densities, and air of different temperatures, act thus upon sound, but air saturated in different degrees, with the vapours of volatile liquids, can be shown by experiment to produce the same effect. Into the path pursued by the carbonic acid in our

first experiment, a flask which I have frequently employed to charge air with vapour was introduced. Through a volatile liquid, partially filling the flask, air was forced into the tunnel $t\ t'$, which was thus divided into spaces containing air saturated with the vapour, and other spaces with air in its ordinary condition. The action of such a medium upon the sound-waves issuing from the bell is very energetic, instantly reducing the violently agitated flame to stillness and steadiness. The removal of the heterogeneous medium instantly restores the noisy flaring of the flame.

A few illustrations of the action of non-homogeneous atmospheres, produced by the saturation of layers of air with the vapours of volatile liquids, may follow here.

Bisulphide of Carbon.—Flame very sensitive, and noisily responsive to the sound. The action of the non-homogeneous atmosphere was prompt and strong, stilling the agitated flame.

Chloroform.—Flame still very sensitive; action similar to the last.

Iodide of Methyl.—Action prompt and energetic.

Amylene.—Very fine action; a short and violently agitated flame was immediately rendered tall and quiescent.

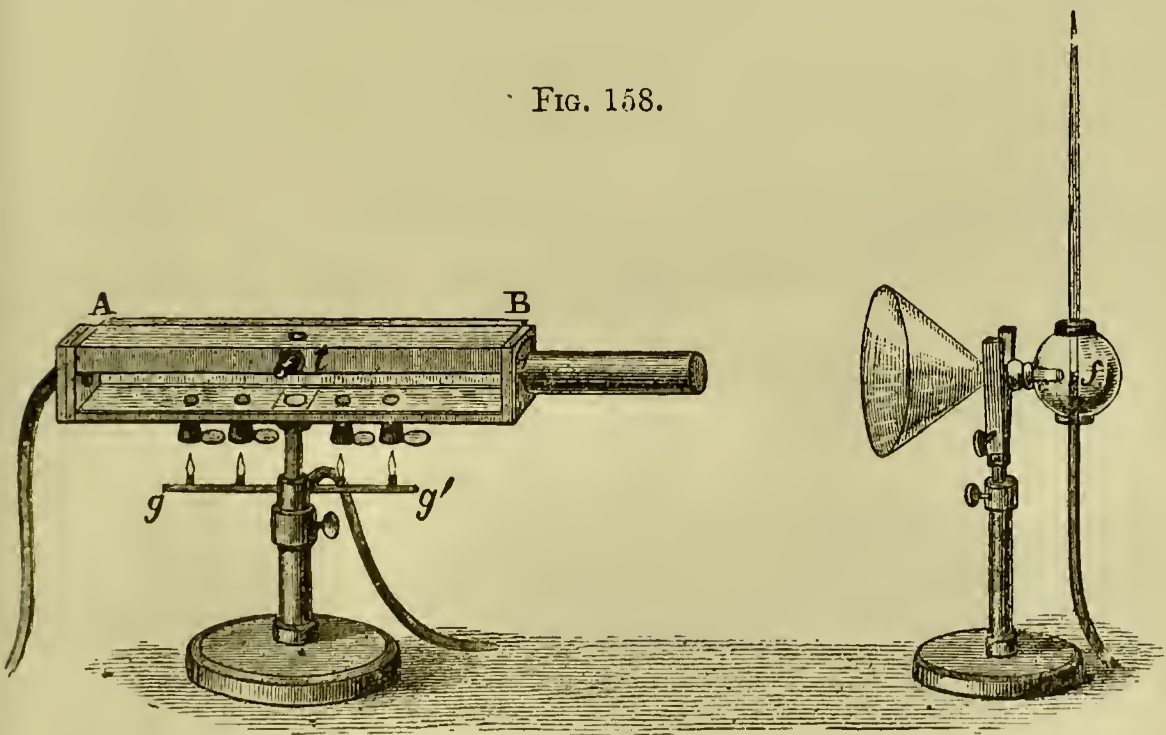
Sulphuric Ether.—Action prompt and energetic.

The vapour of water at ordinary temperatures is so small in quantity, and so attenuated, that it requires special precautions to bring out its action. But with such precautions it was found competent to reduce to quiescence the sensitive flame.

As the skill and knowledge of the experimenter augment he is often able to simplify his experimental combinations. Thus, in the present instance, by the suitable arrangement of the source of sound and the sensitive flame, it was found that not only twenty-five layers, but three or four layers of coal-gas and carbonic acid, sufficed

to still the agitated flame. Nay, with improved manipulation, the action of a single layer of either gas was rendered perfectly sensible. The heated air, moreover, from two or three candle-flames, or even from a single flame, or from a heated poker, was found perfectly competent to stop the flame's agitation. The same remark applies to vapours. Three or four layers of air saturated with the vapour of a volatile liquid stilled the flame; and, by improved manipulation, the action of a single saturated layer could be rendered sensible. In all these cases, more-

Fig. 158.



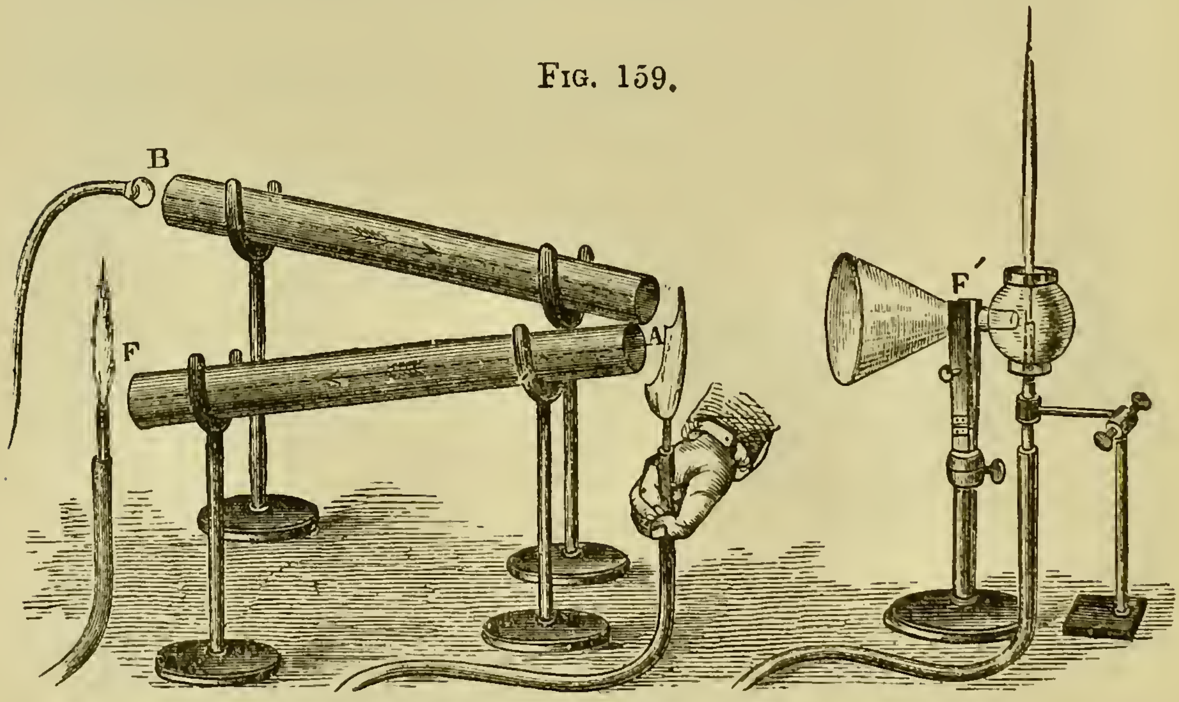
over, a small high-pitched reed might be substituted, with advantage, for the bell.

The simple apparatus sketched in fig. 158 for showing reflection by gases, vapours, and heated air has been devised by my assistant. At the end A of the square pipe A B is a small concertina reed of high pitch, the sound of which violently agitates the sensitive flame *f*. To the horizontal tube *g g′* are attached four small burners, and above them four chimneys through which the heated gases from the flames can ascend into A B. When the coverings of the chimneys are removed and the gas is

ignited, the air within A B is rendered rapidly non-homogeneous, and immediately stills the agitated flame.

The pipe A B may be turned upside down, an orifice seen between A and B fitting on to the stand which supports the tube. The conduit *t* leads into a shallow rectangular box, which communicates by a series of transverse apertures with A B. When air, saturated with the vapour of a volatile liquid, is forced through these apertures, the atmosphere in A B is immediately rendered heterogeneous, the agitated flame being as rapidly stilled.

Fig. 159.



In the experiments at the South Foreland, not only was it proved that the acoustic clouds stopped the sound; but that the sounds which had been refused transmission were sent back by reflection. Wishing to render this echoed sound evident experimentally, I stated to my assistant that we ought to be able to accomplish this. He met my desire by the following beautiful experiment, which has been thus described before the Royal Society:—

'A vibrating reed B (fig. 159) was placed so as to send sound-waves through a tin tube, 38 inches long, and $1\frac{3}{4}$ inch diameter, in the direction B A, the action of the

sound being rendered manifest by its causing a sensitive flame placed at F′ to become violently agitated.

'The invisible heated layer immediately above the luminous portion of an ignited coal-gas flame issuing from an ordinary bat's-wing burner was allowed to stream upwards across the end A of the tin tube. A portion of the sound issuing from the tube was reflected at the limiting surfaces of the heated layer; the part transmitted being now only competent to slightly agitate the sensitive flame at F′.

'The heated layer was then placed at such an angle that the reflected portion of the sound was sent through a second tin tube, A F (of the same dimensions as B A). Its action was rendered visible by causing a second sensitive flame placed at the end of the tube at F to become violently affected. This *echo* continued active as long as the heated layer intervened; but upon its withdrawal the sensitive flame placed at F′, receiving the whole of the direct pulse, became again violently agitated, and at the same moment the sensitive flame at F, ceasing to be affected by the echo, resumed its former tranquillity.

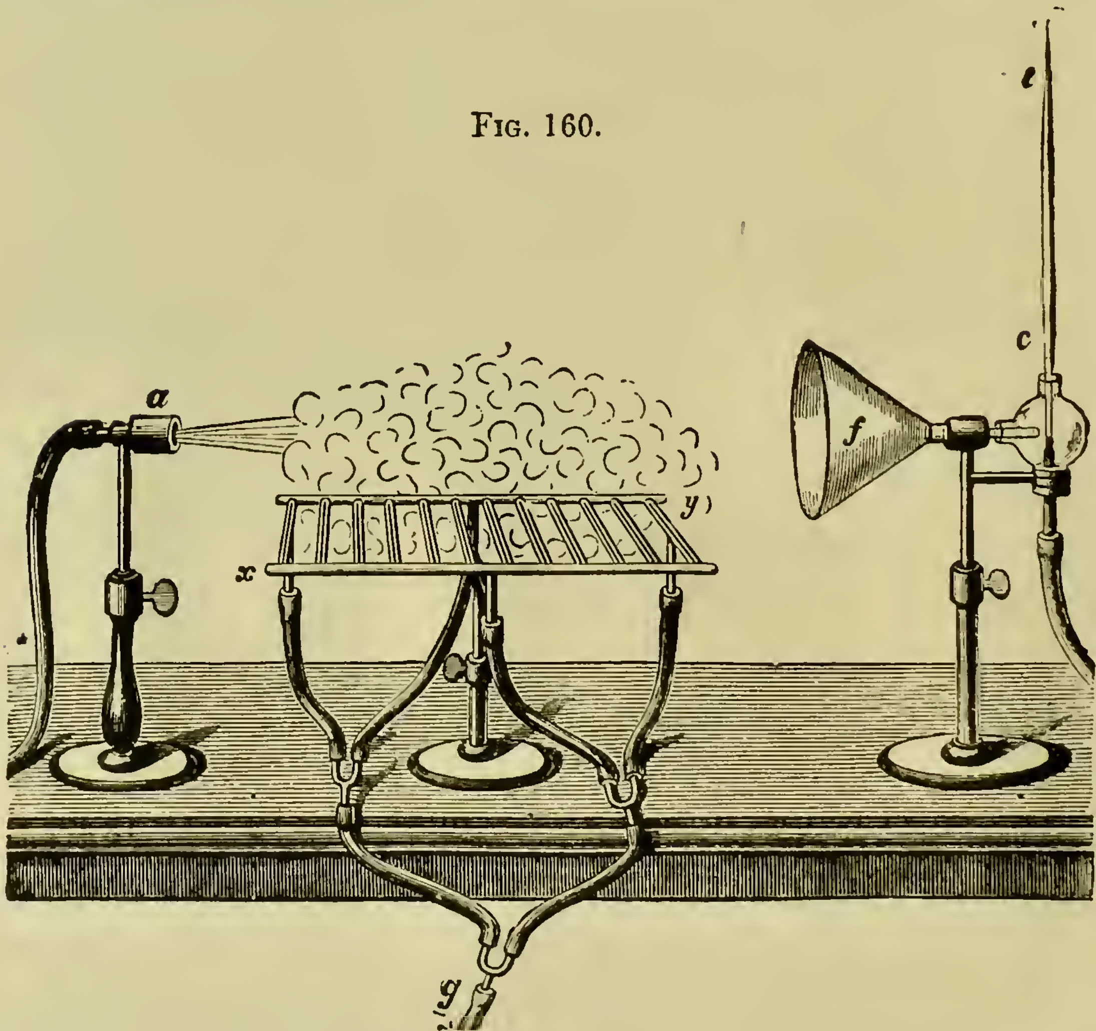
'Exactly the same action takes place when the luminous portion of a gas-flame is made the reflecting layer; but in the experiments above described the invisible layer above the flame only was used. By proper adjustment of the pressure of the gas the flame at F′ can be rendered so moderately sensitive to the direct sound wave that the portion transmitted through the reflecting layer shall be incompetent to affect the flame. Then by the introduction and withdrawal of the bat's-wing flame the two sensitive flames can be rendered alternately quiescent and strongly agitated.

'An illustration is here afforded of the perfect analogy between light and sound; for if a beam of light be projected from B to F′, and a plate of glass be introduced at A in the exact position of the reflecting layer of gas,

the beam will be divided, one portion being reflected in the direction A F, and the other portion transmitted through the glass towards F′, exactly as the sound wave is divided into a reflected and transmitted portion by the layer of heated gas or flame.'

To illustrate still further by experiment the phenomena observed on a large scale at the South Foreland, the following arrangements were devised. *x y*, fig. 160, is a

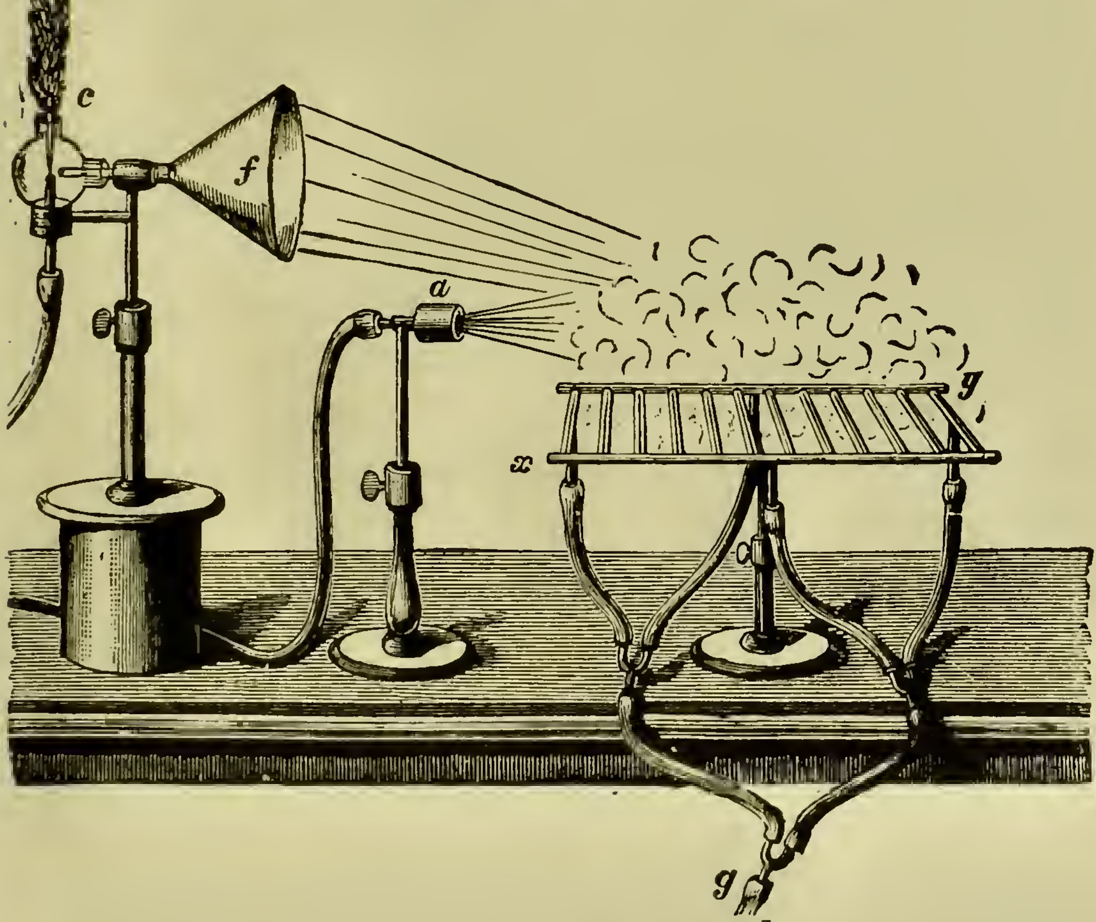
FIG. 160.



rectangle formed like a gridiron, the cross bars of which are brass tubes 0·4 of an inch in diameter, with a slit about $\frac{1}{16}$th of an inch in width running along the top of each. The gridiron is connected by tubes of india-rubber with the gas pipes of the building, so that on turning a cock *g* the gas is distributed pretty uniformly over the rectangle, and issues from the slits. Lighted by a taper, a series of low parallel flames are thus produced. At a

short distance from one end of the gridiron is placed a concertina reed *a*, which when sounded throws the sensitive flame *c* into violent agitation. On igniting the gas, the flame is immediately stilled, through the inability of the sound to traverse the invisible but flocculent air. The opacity of the atmosphere observed on various occasions at the South Foreland is thus strikingly illustrated.

FIG. 161.



Permitting the reed to remain unchanged, the sensitive flame is now removed to a position behind the reed, as in fig. 161. Here the sound is so much enfeebled that the flame can burn there tranquilly when the reed is sounding. Things being in this condition, the gas is lighted and the air above it rendered flocculent. The sound is immediately thrown back in aerial echoes, which possess a force sufficient to strongly agitate the flame *c d*. This is the constant result. When the gas is turned off, after

a few seconds required for the transverse tubes to cool, the flame burns steadily, to be again agitated the moment the invisible acoustic cloud is formed above the gridiron. We have here the aerial echoes of the South Foreland accurately imitated.

Using a second flame to receive the direct sound waves, it flares and roars as long as the gas remains unignited, while the flame behind the reed is still. On igniting the gas the two flames exchange appearances, that which receives the direct sound being stilled, and the other flame being agitated.

The gridiron actually employed in my experiments has a frame 25 inches long and 12 inches wide.[1] It is crossed by 23 tubes, the slits of which are about an inch asunder. A gridiron of half the length and with half the number of slits would be almost equally effectual.

Thus far, then, we have placed our subject in the firm grasp of experiment; nor shall we find this test failing us further on.

PART II.

INVESTIGATION OF THE CAUSES WHICH HAVE HITHERTO BEEN SUPPOSED EFFECTIVE IN PREVENTING THE TRANSMISSION OF SOUND THROUGH THE ATMOSPHERE.

ACTION OF HAIL AND RAIN—ACTION OF SNOW—ACTION OF FOG: OBSERVATIONS IN LONDON—EXPERIMENTS ON ARTIFICIAL FOGS—OBSERVATIONS ON FOGS AT THE SOUTH FORELAND—ACTION OF WIND—ATMOSPHERIC SELECTION—INFLUENCE OF SOUND SHADOW.

§ 1. *Action of Hail and Rain.*

In the first part of this Lecture it was demonstrated that the optic transparency and acoustic transparency of

[1] The tubes constituting the frame have no slits.

our atmosphere are by no means necessarily coincident; that on days of marvellous optical clearness the atmosphere may be filled with impervious acoustic clouds, while days optically turbid may be acoustically clear. We have now to consider, in detail, the influence of the various agents which have hitherto been considered potent in reference to the transmission of sound through the atmosphere.

Falling rain has been credited with the power of seriously obstructing sound. An observation on June 3 has been already referred to as tending to throw doubt on this conclusion. Two other crucial instances will suffice to show its untenability. On the morning of October 8, at 7.45 A.M., a thunderstorm accompanied by heavy rain broke over Dover. But the clouds subsequently cleared away, and the sun shone strongly on the sea. We steamed out. For a time the optical clearness of the atmosphere was extraordinary, but it was acoustically opaque. At 2.30 P.M., a densely black scowl again overspread the heavens to the W.S.W. The distance from the South Foreland being 6 miles, and all hushed on board, the horn was heard very feebly, the syren more distinctly, while the howitzer was heard better than either, though not much superior to the syren.

A squall approached us from the west. In the Alps or elsewhere I have rarely seen the heavens blacker. Vast cumuli floated to the N.E. and S.E.; vast streamers of rain descended in the W.N.W.; huge scrolls of cloud hung in the N.; but spaces of blue were to be seen to the N.N.E.

At 7 miles' distance the syren and horn were both feeble, while the gun sent us a very faint report. A dense shower now enveloped the Foreland.

The rain at length reached us, falling heavily all the way between us and the Foreland; but the sound, instead

of being deadened, rose perceptibly in power. Hail was now added to the rain, and the shower reached a tropical violence, the hailstones floating thickly on the flooded deck. In the midst of this furious squall both the horns and the syren were distinctly heard; and as the shower lightened, thus lessening the local pattering, the sounds so rose in power that we heard them at a distance of 7½ miles distinctly louder than they had been heard through the rainless atmosphere at 5 miles.

At 4 P.M. the rain had ceased and the sun shone clearly through the calm air. At 9 miles' distance the horn was heard feebly, the syren clearly, while the howitzer sent us a loud report. All the sounds were better heard at this distance than they had previously been at 5½ miles; from which, by the law of inverse squares, it follows that the intensity of the sound at 5½ miles' distance must have been augmented at least threefold by the descent of the rain.

On the 23rd of October our steamer had forsaken us for shelter, and I sought to turn the weather to account by making other observations on both sides of the fog-signal station. Mr. Douglass, the Chief Engineer of the Trinity House, was good enough to undertake the observations N.E. of the Foreland; while Mr. Ayres, the Assistant Engineer, walked in the other direction. At 12.50 P.M. the wind blew a gale, and broke into a thunderstorm with violent rain. Inside and outside the Cornhill Coastguard Station, a mile from the instruments in the direction of Dover, Mr. Ayres heard the sound of the syren through the storm; and after the rain had ceased, all sounds were heard distinctly louder than before. Mr. Douglass had sent a fly before him to Kingsdown, and the driver had been waiting for fifteen minutes before he arrived. During this time no sound had been heard, though 40 blasts had been blown in the interval; nor had the coastguard man on duty, a practised observer, heard any of them through-

out the day. During the thunderstorm, and while the rain was actually falling with a violence which Mr. Douglass describes as perfectly torrential, the sounds became audible and were heard by all.

To rain, in short, I have never been able to trace the slightest deadening influence upon sound. The reputed barrier offered by 'thick weather' to the passage of sound was one of the causes which tended to produce hesitation in establishing sound-signals on our coasts. It is to be hoped that the removal of this error may redound to the advantage of coming generations of seafaring men.

§ 2. *Action of Snow.*

Falling snow has been regarded as the most serious obstacle of all to the transmission of sound. We did not extend our observations at the South Foreland into snowy weather; but a previous observation of my own bears directly upon this point. On Christmas night, 1859, I arrived at Chamouni, through snow so deep as to obliterate the road-fences, and to render the labour of reaching the village arduous in the extreme. On the 26th and 27th it fell heavily. On the 27th, during a lull in the storm, I reached the Montanvert, sometimes breast-deep in snow. On the 28th, with great difficulty, two lines of stakes were set out across the glacier, with the view of determining its winter motion. On the 29th the entry in my journal, written in the morning, is, 'Snow, heavy snow; it must have descended through the entire night, the quantity freshly fallen is so great.'

Under these circumstances I planted my theodolite beside the Mer de Glace, having waded to my position through snow which, being dry, reached nearly to my breast. Assistants were sent across the glacier with instructions to measure the displacement of a transverse line of stakes planted previously in the snow. A storm

drifted up the valley, darkening the air as it approached. It reached us, the snow falling more heavily than I had ever seen it elsewhere. It soon formed a heap on the theodolite, and thickly covered my own clothes. Here, then, was a combination of snow in the air, and of soft fresh snow on the ground, such as had not previously been observed; still through such an atmosphere, I was able to make my instructions audible quite across the glacier, the distance being half a mile, while the experiment was rendered reciprocal by one of my assistants making his voice audible to me.

Since the date here referred to I have had various opportunities of testing, under severe conditions, the action of freshly fallen snow upon sound. In 1878, for example, I took to the Alps bells which could be rung by strokes of perfectly definite force. On September 25, my wife took charge of one of these, while I retreated to a distance through thick fog and deep snow combined. We had thus the conjunction of the two agents which have been considered the most hostile to the transmission of sound. Nevertheless the acoustic transparency of the air on this day was extraordinary. On no other occasion did the bell send its vibrations to so great a distance. Under ordinary circumstances a range of 900 yards was held to be considerable; but on the 25th the range was 1300 yards.

§ 3. *Passage of Sound through Textile Fabrics, and through Artificial Showers.*

The flakes upon the Mer de Glace were so thick that it was only at intervals that I was able to pick up the retreating forms of the men. Still the air through which the flakes fell was continuous. Did the flakes merely yield passively to the sonorous waves, swinging like the particles of air themselves to and fro as the sound-waves passed

them? Or did the waves bend by diffraction round the flakes, and emerge from them without sensible loss? Experiment will aid us here by showing the astonishing facility with which sound makes its way among obstacles, and passes through tissues, so long as the continuity of the air in their interstices is preserved.

A piece of millboard or of glass, a plank of wood, or the hand, placed across the open end t' of the tunnel $a\ b\ c\ d$, fig. 157 (page 316), intercepted the sound of the bell, placed in the padded box P, and reduced to stillness the sensitive flame k.

An ordinary cambric pocket-handkerchief, on the other hand, placed across the tunnel-end produced hardly an appreciable effect upon the sound. Through two layers of the handkerchief the flame was strongly agitated; through four layers it was still agitated; while through six layers, though nearly stilled, it was not entirely so.

Dipping the same handkerchief into water, and stretching a single wetted layer across the tunnel-end, it stilled the flame as effectually as the millboard or the wood. Placing the cambric between two leaves of blotting paper, and squeezing it so as to absorb the water, its power of transmission was instantly restored. Hence the conclusion, that the sound-waves passed through the interstices of the cambric.

Through a single layer of thin silk the sound passed without sensible interruption; through six layers the flame was strongly agitated; while through twelve layers the agitation was quite perceptible.

A single layer of this silk, when wetted, stilled the flame.

A layer of soft lint produced but little effect upon the sound: a layer of thick flannel was almost equally ineffectual. Through four layers of flannel the flame was perceptibly agitated. Through a single layer of green baize

the sound passed almost as freely as through air; through four layers of the baize the action was still sensible. Through a layer of close hard felt, half an inch thick, the sound-waves passed with sufficient energy to sensibly agitate the flame. Through 200 layers of cotton-net the sound passed freely. I did not witness these effects without astonishment. Placed against the mouth, through both the felt and the cotton-net air could be drawn. They were, however, quite impervious to light.

A single layer of thin oiled silk stopped the sound and stilled the flame. A leaf of common note-paper, or a five-pound note, also stopped the sound.

The sensitive flame is not absolutely necessary to these experiments. Let a ticking watch be hung six inches from the ear, a cambric handkerchief dropped between it and the ear scarcely sensibly affects the ticking; a sheet of oil-skin or an intensely heated gas column cuts it almost wholly off.

But though oiled silk, foreign post, or a bank-note can stop the sound, a film sufficiently thin to yield freely to the aerial pulses transmits it. A thick soap-film produces an obvious effect upon the sensitive flame; a very thin one does not. The augmentation of the transmitted sound may be observed simultaneously with the generation and brightening of the iridescent colours which indicate the decreasing thickness of the film. A very thin collodion-film acts in the same way.

These experiments may be made with the utmost ease by sounding a small concertina reed at one end of a tin tube, and placing the sensitive flame at a little distance from the other end.

Acquainted with the foregoing facts regarding the passage of sound through cambric, silk, lint, flannel, baize, felt, and cotton-net, you are prepared for the statement that the sound-waves pass without sensible impediment

through heavy artificial showers of rain, hail, and snow. Water-drops, seeds, sand, bran, and flocculi of various kinds, have been employed to form such showers: through all of these, as through the actual rain and hail already described, and through the snow on the Mer de Glace, the sound passes without sensible obstruction.

§ 4. *Action of Fog. Observations in London.*

But the mariner's greatest enemy, fog, is still to be dealt with in detail; and here for a long time the proper conditions of experiment were absent. Up to the end of November we had had frequent days of haze, sufficiently thick to obscure the white cliffs of the Foreland, but no real fog. Still those cases furnished conclusive evidence that the notions entertained regarding the reflection of sound by suspended particles were wrong; for on many days of the thickest haze the sound covered twice the range attained on other days of perfect optical transparency. Such instances dissolved the association hitherto assumed to exist between acoustic transparency and optic transparency, but they left the action of dense fogs undetermined.

On December 9 a memorable fog settled down on London. I addressed a telegram to the Trinity House suggesting some gun-observations. With characteristic promptness came the reply that they would be made in the afternoon at Blackwall. I went to Greenwich in the hope of hearing the guns across the river; but the delay of the train by the fog rendered my arrival too late. Over the river the fog was very dense, and through it came various sounds with great distinctness. The signal-bell of an unseen barge rang clearly out at intervals, and I could plainly hear the hammering at Cubitt's Town, half a mile away, on the opposite side of the river. No deadening of the sound by the fog was apparent.

Through this fog and various local noises, Captain Atkins and Mr. Edwards heard the report of a 12-pounder carronade with a 1-lb. charge far better than the 18-pounder with a 3-lb. charge, an optically clear atmosphere, and all noise absent, was heard on July 3.

Anxious to turn to the best account a phenomenon for which we had waited so long, I tried to grapple with the problem by experiments on a small scale. On the 10th I stationed my assistant with a whistle and organ-pipe on the walk at the south-west end of the bridge dividing Hyde Park from Kensington Gardens. From the eastern end of the Serpentine I heard distinctly both the whistle and the pipe, which produced 380 waves a second. On changing places with my assistant, I heard for a time the distinct blast of the whistle only. The deeper note of the organ-pipe at length reached me, rising sometimes to great distinctness, and sometimes falling to inaudibility. The whistle showed the same intermittence as to period, but in an opposite sense; for when the whistle was faint the pipe was strong, and *vice versâ*. To obtain the fundamental note of the pipe it had to be blown gently, and on the whole the whistle proved the most efficient in piercing the fog.

An extraordinary amount of sound filled the air during these experiments. The resonant roar of the Bayswater and Knightsbridge roads; the clangour of the great bell of Westminster; the railway-whistles, which were frequently blown, and the fog-signals exploded at the various metropolitan stations, were all heard with extraordinary intensity. This could by no means be reconciled with the statements so categorically made regarding the acoustic impenetrability of a London fog.

On the 11th of December, the fog being denser than before, I heard every blast of the whistle, and occasional blasts of the pipe, over the distance between the bridge

and the eastern end of the Serpentine. On joining my assistant at the bridge, the loud concussion of a gun was heard by both of us. A police inspector affirmed that it came from Woolwich, and that he had heard several shots about 2 P.M. and previously. The fact, if a fact, was of the highest importance; so I immediately telegraphed to Woolwich for information. Professor Abel kindly furnished me with the following particulars:—

'The firing took place at 1.40 P.M. The guns proved were of comparatively small size—64-pounders, with 10-lb. charges of powder.

'The concussion experienced at my house and office, about three-quarters of a mile from the butt, was decidedly more severe than that experienced when the heaviest guns are proved with charges of 110 to 120 lbs. of powder. There was a dense fog here at the time of firing.'

These were the guns heard by the police inspector; on subsequent inquiry it was ascertained that two guns were fired at about 3 P.M. These were the guns heard by myself.

Professor Abel also communicated to me the following fact:—'Our workman's bell at the Arsenal Gate, which is of moderate size and anything but clear in tone, is pretty distinctly heard by Professor Bloxam *only* when the wind is *north-east*. During the whole of last week the bell was heard with great distinctness, the wind being *south-westerly* (opposed to the sound). The distance of the bell from Bloxam's house is about three-quarters of a mile as the crow flies.'

Assuredly no question of science ever stood so much in need of revision as this of the transmission of sound through the atmosphere. Slowly but surely we mastered the question; and the further we advanced the more plainly it appeared that our reputed knowledge regarding it was erroneous from beginning to end.

On the morning of the 12th the fog attained its maxi-

mum density. It was not possible to read at my window, which fronted the open western sky. At 10.30 I sent an assistant to the bridge, and listened for his whistle and pipe at the eastern end of the Serpentine. The whistle rose to a shrillness far surpassing anything previously heard, but it sank sometimes almost to inaudibility; indicating that though the air was on the whole highly homogeneous, acoustic clouds still drifted through the fog. A second pipe, which was quite inaudible yesterday, was plainly heard this morning. We were able to discourse across the Serpentine to-day with much greater ease than yesterday.

During our summer observations we had once or twice been able to fix the position of the Foreland in thick haze by the direction of the sound. To-day my assistant, hidden by the fog, walked up to the Watermen's Boathouse sounding his whistle; and I walked along the opposite side of the Serpentine, clearly appreciating for a time that the line joining us was oblique to the axis of the river. Coming to a point which seemed to be exactly abreast of him, I marked it; and on the following day, when the fog had cleared away, the marked position was found to be perfectly exact. When undisturbed by echoes, the ear, with a little practice, becomes capable of fixing with great precision the direction of a sound. This is a point of considerable difficulty, which, though examined by Lord Rayleigh and others, is hardly yet cleared up.

On reaching the Serpentine this morning a peal of bells, which then began to ring, seemed so close at hand that it required some reflection to convince me that they were ringing to the north of Hyde Park. The sounds fluctuated wonderfully in power. Prior to the striking of eleven by the great bell of Westminster, a nearer bell struck with loud clangour. The first five strokes of the Westminster bell were afterwards heard, one of them being ex-

tremely loud; but the last six strokes were inaudible. An assistant was stationed to attend to the 12 o'clock bells. The clock which had struck so loudly at 11 was unheard at 12, while of the Westminster bell eight strokes out of twelve were inaudible. To such singular changes is the atmosphere liable.

At 7 P.M. the Westminster bell striking seven was not at all heard from the Serpentine, while the nearer bell already alluded to was heard distinctly. The fog had cleared away, and the lamps on the bridge could be seen from the eastern end of the Serpentine burning brightly; but instead of the sound sharing the improvement of the light, what might be properly called an acoustic fog took the place of its optical predecessor. Several series of the whistle and organ-pipe were sounded in succession; one series only of the whistle-sounds was heard, all the others being quite inaudible. Three series of the organ-pipe were heard, but exceedingly faintly. On reversing the positions and sounding as before, nothing whatever was heard.

At 8 o'clock the chimes and hour-bell of the Westminster clock were both very loud. The 'acoustic fog' had disappeared.

Extraordinary fluctuations were also observed in the sound of the church bells: in a few seconds they would sink from a loudly ringing peal into utter silence, from which they would rapidly return to loud-tongued audibility. The intermittent drifting of fog over the sun's disc (by which his light is at times obscured, at times revealed) is the optical analogue of these effects. As regards such changes, the acoustic deportment of the atmosphere is a true transcript of its optical deportment.

At 9 P.M. three strokes only of the Westminster clock were heard; the others were inaudible. The air had relapsed in part into its condition at 7 P.M., when all the

strokes were unheard. The quiet of the park this evening, as contrasted with the resonant roar which filled the air on the two preceding days, was very remarkable. The sound, in fact, was stifled in the optically clear but acoustically flocculent atmosphere.

On the 13th, the fog being displaced by thin haze, I went again to the Serpentine. The carriage sounds were damped to an extraordinary degree. The roar of the Knightsbridge and Bayswater roads had subsided, the tread of troops which passed us a little way off was unheard, while at 11 A.M. both the chimes and the hour-bell of the Westminster clock were stifled. Subjectively considered, all was favourable to auditory impressions; but the very cause that damped the local noises extinguished our experimental sounds. The voice across the Serpentine to-day, with my assistant plainly visible in front of me, was distinctly feebler than it had been when each of us was hidden from the other in the densest fog.

Placing the source of sound at the eastern end of the Serpentine, I walked along its edge from the bridge towards the end. The distance between these two points is about 1,000 paces. After 500 had been stepped, the sound was not so distinct as it had been at the bridge on the day of densest fog. The optical cleansing of the air through the melting away of the fog had, therefore, so darkened it acoustically, that a sound generated at the eastern end of the Serpentine was lowered, at a point midway between the end and the bridge, to one-fourth of its intensity.

To these observations one or two subsequent ones may be added. On several of the moist and warm days at the beginning of 1874, I stood at noon beside the railing of St. James's Park, near Buckingham Palace, three-quarters of a mile from the clock tower, which was clearly visible. Not a single stroke of 'Big Ben' was heard. On January

19 fog and drizzling rain obscured the tower; still from the same position I not only heard the strokes of the great bell, but also the chimes of the quarter bells.

During the exceedingly dense and 'dripping' fog of January 22, from the same railings, I heard every stroke of the bell. At the end of the Serpentine, when the fog was densest, the Westminster bell was heard striking loudly eleven. Towards evening this fog began to melt away, and at 6 o'clock I went to the end of the Serpentine to observe the effect of the optical clearing upon the sound. Not one of the strokes reached me. At 9 o'clock and at 10 o'clock my assistant was in the same position, and on both occasions he failed to hear a single stroke of the bell. It was a case precisely similar to that of December 13, when the dissolution of the fog was accompanied by a decided acoustic thickening of the air.[1]

§ 5. *Observations at the South Foreland.*

Satisfactory and indeed conclusive as these results seemed, I desired exceedingly to confirm them by experiments with the instruments actually employed at the South Foreland. On the 10th of February I had the gratification of receiving the following note and inclosure from the Deputy Master of Trinity House:—

'My dear Tyndall,—The inclosed will show how accurately your views have been verified, and I send them on at once without waiting for the details. I think you will be glad to have them, and as soon as I get the report it shall be sent to you. I made up my mind ten days ago that there would be a chance in the light foggy-disposed weather at home, and therefore sent the *Argus* off at an

[1] A friend informs me that he has followed a pack of hounds on a clear calm day without hearing a single yelp from the dogs; while on calm foggy days from the same distance the musical uproar of the pack was loudly audible.

February. Was it the 7th February (see 3rd line from bottom.)

hour's notice, and requested the Fog Committee to keep one member on board. On Friday I was so satisfied that the fog would occur that I sent Edwards down to record the observations. . . .

'Very truly yours,
'FRED. ARROW.'

The inclosure referred to consisted of notes from Captain Atkins and Mr. Edwards. Captain Atkins wrote thus:—

'As arranged, I came down here Dover by the mail express, meeting Mr. Edwards at Cannon Street. We put up at the "Dover Castle," and next morning at 7 I was awoke by sounds of the syren. On jumping up I discovered that the long-looked-for fog had arrived, and that the *Argus* had left her moorings.

'However, had I been on board, the instructions I left with Troughton (the master of the *Argus*) could not have been better carried out. About noon the fog cleared up and the *Argus* returned to her moorings, when I learned that they had "taken both syren and horn sounds" to a distance of 11 miles from the station, where they dropped a buoy. This I knew to be correct, as I have this morning recovered the buoy, and the distances both in and out agree with Troughton's statement. I have also been to the Varne light-ship ($12\frac{3}{4}$ miles from the Foreland), and ascertained that during the fog of Saturday forenoon they "distinctly" heard the sounds.'

Mr. Edwards, who was constantly at my side during our summer and autumn observations, and who is thoroughly competent to form a comparative estimate of the strength of the sounds, stated that those of the 7th were 'extraordinarily loud,' both Captain Atkins and himself being awoke by them. He did not remember ever before hearing the sounds so loud in Dover; it

seemed as though the observers were close to the instruments.

Other days of fog preceded this one, and they were all days of acoustic transparency, the day of densest fog being acoustically the clearest of all.

The results here recorded are of the highest importance, for they bring us face to face with a dense fog and an actual fog-signal, and confirm in the most conclusive manner the observations made in London.

It is exceedingly interesting to compare the transmission of sound on February 7 with its transmission on October 14. The wind on both days had the same strength and direction. My notes of the observations show the latter to have been, throughout, a day of extreme optical clearness. The range was 10 miles. During the fog of February 7, the *Argus* heard the sound at 11 miles; and it was also heard at the Varne light-vessel, which is 12¾ miles from the Foreland.

It is also worthy of note that through the same fog the sounds were well heard at the South Sand Head light-vessel, which is in the opposite direction from the South Foreland, and was actually behind the syren. This important circumstance is to be borne in mind: on February 7 the syren happened to be pointed, not towards the *Argus*, but towards Dover. Had the yacht been in the axis of the instrument, it is almost certain that the sound would have been heard all the way across to the coast of France.

It is hardly necessary for me to say a word to guard myself against the misconception that I consider sound to be assisted by the fog itself. The fog particles have no more influence upon the waves of sound than the suspended particles stirred up over the banks of Newfoundland have upon the waves of the Atlantic. A homogeneous air is the usual associate of fog, and hence the acoustic clearness of foggy weather.

§ 6. *Experiments on Artificial Fogs.*

These observations are clinched and finished by being brought within the range of laboratory experiment. Here we shall learn incidentally a lesson as to the caution required from an experimenter.

The smoke from smouldering brown paper was allowed to stream upwards through its rectangular apertures, into the tunnel *t t′* (fig. 157); the action upon the sound-waves was strong, rendering the short and agitated sensitive flame *k* tall and quiescent.

Air first passed through ammonia, then through hydrochloric acid, and thus loaded with thick fumes, was sent into the tunnel; the agitated flame was rendered immediately quiescent, indicating a very decided action on the part of the artificial fog.

Air passed through perchloride of tin and sent into the tunnel produced exceedingly dense fumes. The action upon the sound-waves was very strong. is suppressing agita

The dense smoke of resin, burnt before the open end of the tunnel, and blown into it with a pair of bellows, had also the effect of stopping the sound-waves, so as to still the agitated flame.

The conclusion seems clear; and its perfect harmony with the prevalent *à priori* notions as to the action of fog upon sound makes it almost irresistible. But caution is here necessary. The smoke of the brown paper was *hot*; the flask containing the hydrochloric acid was *hot*; that containing the perchloride of tin was *hot*; while the resin-fumes produced by a red-hot poker were also obviously hot. Were the results, then, due to the fumes or to the differences of temperature? The observations might well have proved a trap to an incautious reasoner.

Instead of the smoke and heated air, the heated air alone from four red-hot pokers was permitted to stream upwards

into the tunnel; the action on the sound-waves was very decided, though the tunnel was optically empty. The flame of a candle was placed at the tunnel end, and the hot air just above its tip was blown into the tunnel; the action on the sensitive flame was decided. A similar effect was produced when the air, ascending from a red-hot iron, was blown into the tunnel.

In these latter cases the tunnel remained optically clear, while the same effect as that produced by the resin, smoke, and fumes, was observed. Clearly, then, we are not entitled to ascribe, without further investigation, to the artificial fog an effect which may have been due to the air which accompanied it.

Having eliminated the fog and proved the non-homogeneous air effective, our reasoning will be completed by eliminating the heat, and proving the fog ineffective.

Instead of the tunnel *t t′*, fig. 157, a cupboard with glass sides, 3 feet long, 2 feet wide, and about 5 feet high, was filled with fumes of various kinds. Here it was thought the fumes might remain long enough for differences of temperature to disappear. Two apertures were made in two opposite panes of glass 3 feet asunder. In front of one aperture was placed the bell in its padded box, and behind the other aperture, and at some distance from it, the sensitive flame.

Phosphorus placed in a cup floating on water was ignited within the closed cupboard. The fumes were so dense that considerably less than the three feet traversed by the sound extinguished totally a bright candle-flame. At first there was a slight action upon the sound; but this rapidly vanished, the flame being no more affected than if the sound had passed through pure air. The first action was manifestly due to differences of temperature, and it disappeared when the temperature was equalised.

The cupboard was next filled with the dense fumes of

gunpowder. At first there was a slight action; but this disappeared even more rapidly than in the case of the phosphorus, the sound passing as if no fumes were there. It required less than half a minute to abolish the action in the case of the phosphorus, but a few seconds sufficed in the case of the gunpowder. These fumes were far more than sufficient to quench the candle-flame.

The dense smoke of resin, when the temperature had become equable, exerted no action on the sound.

The fumes of gum mastic were equally ineffectual.

The fumes of the perchloride of tin, though of extraordinary density, exerted no sensible effect upon the sound.

Exceedingly dense fumes of chloride of ammonium next filled the cupboard. A fraction of the space through which the sound passed sufficed to quench the candle-flame. Soon after the cupboard was filled, the sound passed without the least sensible deterioration. An aperture at the top of the cupboard was opened; but though a dense smoke-column ascended through it, many minutes elapsed before the candle-flame could be seen through the attenuated fog.

Steam from a copper boiler was so copiously admitted into the cupboard as to fill it with a dense cloud. No real cloud was ever so dense; still the sound passed through it without the least sensible diminution. This being the case, cloud-echoes are not a likely phenomenon.

In all of these cases, when a couple of Bunsen's burners were ignited within the cupboard containing the fumes, less than a minute's action rendered the air so heterogeneous that the sensitive flame was completely stilled. x

These acoustically inactive fogs were subsequently proved competent to cut off the electric light. i.e. intercept

Experiment and observation go, therefore, hand in hand in demonstrating that fogs have no sensible action upon

= non-homogeneous, i.e. of miscellaneous composition

sound. The notion of their impenetrability, which so powerfully retarded the introduction of phonic coast signals, being thus abolished, we have solid ground for the hope that disasters due to fogs and thick weather will in the future be materially mitigated.

§ 7. *Action of Wind.*

In stormy weather we were frequently forsaken by our steamer, which had to seek shelter in the Downs or in Margate Roads, and on such occasions the opportunity was turned to account to determine the effect of the wind. On October 11, accompanied by Mr. Douglass and Mr. Edwards, I walked along the cliffs from Dover Castle towards the Foreland, the wind blowing strongly against the sound. About a mile and a half from the Foreland, we first heard the faint but distinct sound of the syren. The horn-sound was inaudible. A gun fired during our halt was also unheard.

As we approached the Foreland we saw the smoke of a gun. Mr. Edwards heard a faint crack, but neither Mr. Douglass nor I heard anything. The sound of the syren was at the same time of piercing intensity. We waited for ten minutes, when another gun was fired. The smoke was at hand, and I thought I heard a faint thud, but could not be certain. My companions heard nothing. On pacing the distance afterwards we found it to be only 550 yards. We were shaded at the time by a slight eminence from both the syren and the gun, but this could not account for the utter extinction of the gun-sound at so short a distance, and at a time when the syren sent to us a note of great power.

Mr. Ayres, at my request, walked to windward along the cliff, while Mr. Douglass proceeded to St. Margaret's Bay. During their absence I had 3 guns fired. Mr.

Ayres heard only one of them. Favoured by the wind, Mr. Douglass, at twice the distance, and far more deeply immersed in the sound-shadow, heard all three reports with the utmost distinctness.

Joining Mr. Douglass, we continued our walk to a distance of three-quarters of a mile beyond St. Margaret's Bay. Here, being dead to leeward, though the wind blew with unabated violence, the sound of the syren was borne to us with extraordinary power.[1] In this position we also heard the gun loudly, and two other loud reports at the proper interval of ten minutes, as we returned to the Foreland.

It is within the mark to say that the gun on October 11 was heard five times and might probably have been heard fifteen times as far to leeward as to windward.

In windy weather the shortness of its sound is a serious drawback to the use of the gun as a signal. In the case of the horn and syren, time is given for the attention to be fixed upon the sound; and a single puff, while cutting out a portion of the blast, does not obliterate it wholly. Such a puff, however, may be fatal to the momentary gun-sound.

On the leeward side of the Foreland, on the 23rd of October, the sounds were heard at least four times as far as on the windward side, while in both directions the syren possessed the greatest penetrative power.

On the 24th the wind shifted to E.S.E., and the sounds, which when the wind was W.S.W. failed to reach Dover, were now heard in the streets through thick rain. On the 27th the wind was E.N.E. In our writing-room in the Lord Warden Hotel, in the bedrooms, and on the staircase the sound of the syren reached us with surprising power, piercing through the whistling and moaning of the

[1] The horn here was temporarily suspended, but doubtless would have been well heard.

wind, which blew through Dover towards Folkestone. The sounds were heard by Mr. Edwards and myself at 6 miles from the Foreland on the Folkestone road; and had the instruments not then ceased sounding, they might have been heard much further. At the South Sand Head light-vessel, $3\frac{3}{4}$ miles on the opposite side, no sound had been heard throughout the day. On the 28th, the wind being N. by E., the sounds were heard in the middle of Folkestone, 8 miles off, while in the opposite direction they failed to reach $3\frac{3}{4}$ miles. On the 29th the limits of range were Eastware Bay on the one side and Kingsdown on the other; on the 30th the limits were Kingsdown on the one hand and Folkestone Pier on the other. With a wind having a force of 4 or 5 it was a very common observation to hear the sound in one direction three times as far as in the other. With such results before me, which probably surpass in number, variety, and definiteness any previously obtained, I am not likely to underrate the influence of wind upon sound.

The action of the wind here illustrated remained long an enigma. I well remember hearing Sir John Herschel—the author of the most important essay on Sound then extant—express his wonder at the differences in sonorous power of the Hawkhurst church bells, according as the direction of the sound coincided with, or was opposed to, the direction of the wind. He shook his head in intimation that the problem was still unsolved. In the year 1857, however, the difficulty had been conquered by Professor Stokes.

His explanation is this. In the case of wind the translation of the air close to the earth's surface is slower than at a distance above the surface, because the air is held back by its friction against the earth and by internal friction. Let the momentary position of a small portion

of a sound wave moving against the wind near the ground, be represented by a vertical line. The top of the wave, which is furthest removed from the earth's surface, is pushed back by the moving air more than the bottom, which is opposed by a wind of less velocity. The wave therefore soon ceases to be vertical, its upper portion leaning backwards. Now, the motion of a sound wave is always at right angles to its front, and a moment's reflection will show that a wave caused by the wind to lean in the manner described, will move, not horizontally, but obliquely upwards.

The proof of this theory turned out to be far more easy than was at first supposed, and to Professor Osborne Reynolds we are indebted for its experimental verification. His source of sound was a small electric bell capable of being raised and lowered. Sounding this bell in windy weather, he moved to windward, and found that at a certain distance the sonorous waves passed over his head exactly as indicated by Stokes's theory. He also found that by raising the bell, its sound was heard further against the wind than when it was placed near the ground. I have myself made many experiments on this subject on Wimbledon Common, and verified the results of Professor Reynolds. An arrangement was devised whereby a bell could be drawn up by means of a pulley from the ground to a height of nine feet. Sounded below by a stroke, the strength of which was fixed by an adjusted spring, an observer retreated to windward until the sound ceased to be heard. The bell was then raised and again struck. The sound was immediately heard, the raising of the bell having to some extent neutralised the tilting up of the waves of sound. Again, by means of a hinged ladder planted upon the Common, I was enabled to raise my head to a height of ten or twelve feet above the ground. Placing the ladder to windward, the bell was sounded and

caused to retreat until an ear placed near the ground ceased to hear anything. On ascending the ladder, the deflected waves were recovered, the sound becoming distinctly audible. The results were verified by experiments on a larger scale made in the Pagoda of Kew Gardens.

It is obvious that any other cause which produces a difference of motion of the different parts of a sound wave will have an effect similar to that produced by the wind. Professor Osborne Reynolds had the penetration to discern that differences of temperature might come into play here. Supposing, for example, the air close to the earth's surface to be warmer than that above it, the end of a sound wave close to the ground would, in still weather, move more rapidly than the parts of the wave at some height above it. In this way a wave at first vertical would become oblique, and, in consequence of its obliquity, would, as in the case of an opposing wind, be tilted upwards. Professor Osborne Reynolds is here dealing with a true cause, but he will, I trust, excuse me if I cannot, in all cases, accept the explanations which he has founded upon it.

§ 8. *Atmospheric Selection.*

It has been stated that the atmosphere, on different days, shows preferences to different sounds. This point is worthy of further illustration.

After the violent shower which passed over us on October 18, the sounds of all the instruments, as already stated, rose in power; but it was noticed that the horn sound, which was of lower pitch than that of the syren, improved most, at times not only equalling, but surpassing, the sound of its rival. From this it might be inferred that the atmospheric change produced by the rain favoured more especially the transmission of the longer sonorous waves.

But our programme enabled us to go further than mere inference. It had been arranged on the day mentioned, that up to 3.30 P.M. the syren should perform 2,400 revolutions a minute, generating 480 waves a second. As long as this rate continued, the horn, after the shower, had the advantage. The rate of rotation was then changed to 2,000 a minute, or 400 waves a second, when the syren sound immediately surpassed that of the horn. A clear connection was thus established between aerial reflection and the length of the sonorous waves.

The 10-inch Canadian whistle being capable of adjustment so as to produce sounds of different pitch, on the 10th of October I ran through a series of its sounds. The shrillest appeared to possess great intensity and penetrative power. The belief is common that a note of this character (which affects so powerfully, and even painfully, an observer close at hand) has also the greatest range. Mr. A. Gordon, in his examination before the Committee on Lighthouses, in 1845, expressed himself thus:—'When you get a shrill sound, high in the scale, that sound is carried much further than a lower note in the scale.' I have heard the same opinion expressed by other scientific men.

On the 14th of October the point was submitted to an experimental test. It had been arranged that up to 11.30 A.M. the Canadian whistle, which had been heard with such piercing intensity on the 10th, should sound its shrillest note. At the hour just mentioned we were beside the Varne buoy, 7¾ miles from the Foreland. The syren, as we approached the buoy, was heard through the paddle noises; the horns were also heard, but more feebly than the syren. We paused at the buoy and listened for the 11.30 gun. Its boom was heard by all. Neither before nor during the pause was the shrill-sounding Canadian whistle once heard. At the appointed time it

was adjusted to produce its ordinary low-pitched note, which was immediately heard. Further out the low boom of the cannon continued audible after all the other sounds had ceased.

But it was only during the early part of the day that this preference for the longer waves was manifested. At 3 P.M. the case was completely altered, for then the high-pitched syren was heard when all the other sounds were inaudible. On many other days we had illustrations of the varying comparative power of the syren and the gun. On the 9th of October sometimes the one, sometimes the other was predominant. On the morning of the 13th the syren was clearly heard on Shakespeare's Cliff, while two guns with their puffs perfectly visible were unheard. On October 16, 2 miles from the signal-station, the gun at 11 o'clock was inferior to the syren, but both were heard. At 12.30, the distance being 6 miles, the gun was quite unheard, while the syren continued faintly audible. Later on in the day the experiment was twice repeated. The puff of the gun was in each case seen, but nothing was heard. In the last experiment, when the gun was quenched, the syren sent forth a sound so strong as to maintain itself through the paddle noises. The day was clearly hostile to the passage of the longer sonorous waves.

October 17 began with a preference for the shorter waves. At 11.30 A.M. the mastery of the syren over the gun was pronounced; at 12.30 the gun slightly surpassed the syren; at 1, 2, and 2.30 P.M. the gun also asserted its mastery. This preference for the longer waves was continued on October 18. On October 20 the day began in favour of the gun, then both became equal, and finally the syren gained the mastery; but the day had become stormy, and a storm is always unfavourable to the momentary gun sound. The same remark applies to the experiments of October 21. At 11 A.M., distance 6½ miles,

when the syren made itself heard through the noises of wind, sea, and paddles, the gun was fired; but, though listened for with all attention, no sound was heard. Half an hour later the result was the same. On October 24 five observers saw the flash of the gun at a distance of 5 miles, but heard nothing; all of them at this distance heard the syren distinctly; a second experiment on the same day yielded the same result. On the 27th also the syren was triumphant; and on three several occasions on the 29th its mastery over the gun was very decided. Such experiments yield new conceptions as to the scattering of sound in the atmosphere.

§ 9. *Concluding Remarks.*

A few additional remarks and suggestions will fitly wind up this chapter. It has been proved that in some states of the weather the howitzer firing a 3-lb. charge commands a larger range than the whistles, trumpets, or syren. This was the case, for example, on the particular day, October 17, when the ranges of all the sounds reached their maximum.

On many other days, however, the inferiority of the gun to the syren was demonstrated in the clearest manner. The gun puffs were seen with the utmost distinctness at the Foreland, but no sound was heard, the note of the syren at the same time reaching us with distinct and considerable power.

The disadvantages of the gun are these:—

a. The duration of the sound is so short that, unless the observer is prepared beforehand, the sound, through lack of attention rather than through its own powerlessness, is liable to be unheard.

b. Its liability to be quenched by a local sound is so great that it is sometimes obliterated by a puff of wind

taking possession of the ears at the time of its arrival This point was alluded to by Arago, in his report on the celebrated experiments of 1822. By such a puff a momentary gap is produced in the case of a continuous sound, but not entire extinction.

c. Its liability to be quenched or deflected by an opposing wind, so as to be practically useless at a very short distance to windward, is very remarkable. A case has been cited in which the gun failed to be heard against a violent wind at a distance of 550 yards from the place of firing, the sound of the syren at the same time being heard with great intensity.

Still, notwithstanding these drawbacks, I think the gun is entitled to rank as a first-class signal. I have had occasion myself to observe its extreme utility at Holyhead and the Kish light-vessel near Kingstown. The commanders of the Holyhead boats, moreover, are unanimous in their commendation of the gun. An important addition in its favour is the fact that, in fog, the flash or glare often comes to the aid of the sound. On this point the evidence is quite conclusive.

There may be cases in which the combination of the gun with one of the other signals may be desirable. Where it is wished to confer an unmistakable individuality on a fog-signal station, such a combination might with advantage be resorted to.

If the gun be retained as one form of fog signal (and I should be sorry at present to recommend its total abolition), it ought to be of the most suitable description. Our experiments prove the sound of the gun to be dependent on its shape; but we do not know that we have employed the best shape. This suggests the desirability of constructing a gun with special reference to the production of sound.

Still more important than the shape of the gun is the

quality of the powder. A sharp shock is requisite to the production of a sonorous wave: a quick-burning powder is therefore most suitable. Gun-cotton or dynamite surpasses gunpowder as a sound-producer.

An absolutely uniform superiority on all days cannot be conceded to any one of the instruments subjected to examination; still, our observations have been so numerous and long-continued as to enable us to come to the sure conclusion that, on the whole, the steam syren is the most powerful fog signal which has hitherto been tried in England. It is specially powerful when local noises, such as those of wind, rigging, breaking waves, shore surf, and the rattle of pebbles, have to be overcome. Its density, quality, pitch, and penetration, render it dominant over such noises after all other signal-sounds have succumbed.

I have not, therefore, hesitated to recommend the introduction of the syren as a coast signal.

It will be desirable in each case to confer upon the instrument a power of rotation, so as to enable the person in charge of it to point its trumpet against the wind or in any other required direction. This arrangement was made at the South Foreland, and it presents no mechanical difficulty. It is also desirable to mount the syren so as to permit of the depression of its trumpet fifteen or twenty degrees below the horizon.

In selecting the position at which a fog signal is to be mounted, the possible influence of a sound shadow, and the possible extinction of the sound by the interference of the direct waves with waves reflected from the shore, must form the subject of the gravest consideration. Preliminary trials may, in most cases, be necessary before fixing on the precise point at which the instrument is to be placed.

No fog signal hitherto tried is able to fulfil the condi-

tion laid down in a very able letter already referred to, namely, '*that all fog signals should be distinctly audible for at least 4 miles, under every circumstance.*' Circumstances may exist to prevent the most powerful sound from being heard at half this distance. What may with certainty be affirmed is, that in almost all cases the syren may certainly be relied on at a distance of 2 miles; in the great majority of cases it may be relied upon at a distance of 3 miles, and in the majority of cases to a distance greater than 3 miles.

Happily the experiments thus far made are perfectly concurrent in indicating that, at the particular time when fog signals are needed, the air holding the fog in suspension is in a highly homogeneous condition; hence it is in the highest degree probable that, in the case of fog, we may rely upon the signals being effective at greater distances than those just mentioned.

I am cautious not to inspire the mariner with a confidence which may prove delusive. When he hears a fog signal he ought, as a general rule (at all events until extended experience justifies the contrary), to assume the source of sound to be not more than 2 or 3 miles distant, and to heave his lead or take other necessary precautions. If he errs at all in his estimate of distance, it ought to be on the side of safety.

With the instruments now at our disposal wisely established along coasts, I venture to think that the saving of property in ten years will be an exceedingly large multiple of the outlay necessary for the establishment of such signals. The saving of life appeals to the higher motives of humanity.

In a report written for the Trinity House on the subject of fog signals, my excellent predecessor, Professor Faraday, expresses the opinion that a false promise to the mariner would be worse than no promise at all. Casting

our eyes back upon the observations here recorded, we find the sound range on clear calm days varying from $2\frac{1}{2}$ miles to $16\frac{1}{2}$ miles. It must be evident that an instruction founded on the latter observation would be fraught with peril in weather corresponding to the former. Not the maximum but the minimum sound range should be impressed upon the mariner. Want of attention to this point may be followed by disastrous consequences.

This remark is not made without cause. I have before me a 'Notice to Mariners' regarding a fog whistle recently mounted at Cape Race, which is reputed to have a range of 20 miles in calm weather, 30 miles with the wind, and in stormy weather or against the wind 7 to 10 miles. Now, considering the distance reached by sound in our observations, I should be willing to concede the possibility, in a more homogeneous atmosphere than ours, of a sound range on *some* calm days of 20 miles, and on *some* light windy days of 30 miles, to a powerful whistle; but I entertain a strong belief that the stating of these distances, or of the distance 7 to 10 miles against a storm, without any qualification, is calculated to inspire the mariner with false confidence. I would venture to affirm that at Cape Race calm days might be found in which the range of the sound will be less than one-third of what this notice states it to be. Such publications ought to be without a trace of exaggeration, and furnish only data on which the mariner may with perfect confidence rely. My object in extending these observations over so long a period was to make evident to all how fallacious it would be, and how mischievous it might be, to draw general conclusions from observations made in weather of great acoustic transparency.

Thus ends, for the present at all events, an inquiry which I trust will prove of some importance, scientific as well as practical. In conducting it I have had to con-

gratulate myself on the unfailing aid and co-operation of the Elder Brethren of the Trinity House. Captain Drew, Captain Close, Captain Were, Captain Atkins, and the Deputy Master, have all from time to time taken part in the inquiry. To the eminent Arctic navigator, Admiral Collinson, who showed throughout unflagging and, I would add, philosophic interest in the investigation, I am indebted for most important practical aid. He was almost always at my side, comparing opinions with me, placing the steamer in the required positions, and making with consummate skill and promptness the necessary sextant observations. I am also deeply sensible of the important services rendered by Mr. Douglass, the able and indefatigable Engineer, by Mr. Ayres, the Assistant Engineer, and by Mr. Price Edwards, then Private Secretary of the Deputy Master of the Trinity House.

The officers and gunners at the South Foreland also merit my best thanks, as also Mr. Holmes and Mr. Laidlaw, who had charge of the trumpets, whistles, and syren.

In the subsequent experimental treatment of the subject I have been most ably aided by my excellent assistant, Mr. John Cottrell.

NOTE.

In the Appendix will be found a brief paper on 'Acoustic Reversibility, in which additional experiments are described. There, also, will be found described two cases of acoustic opacity which rank amongst the most remarkable hitherto recorded.

See b.

SUMMARY OF LECTURE VII.

The seventh Lecture contains an account of researches on the acoustic qualities of the atmosphere in relation to the question of fog signals, and of the laboratory experiments suggested by these researches.

The previous condition of the question is first described; after which comes a description of the instruments and appliances established in 1873 at the South Foreland, with a view to the exhaustive examination of the subject. The investigation was conducted at the expense of the Government, and under the auspices of the Elder Brethren of the Trinity House.

The most conflicting results were at first obtained. On the 19th of May, 1873, the sound range was $3\frac{1}{3}$ miles; on the 20th it was $5\frac{1}{2}$ miles; on the 2nd of June, 6 miles; on the 3rd, more than 9 miles; on the 10th, 9 miles; on the 25th, 6 miles; on the 26th, $9\frac{1}{4}$ miles; on the 1st of July, $12\frac{3}{4}$ miles; on the 2nd, 4 miles; while on the 3rd, with a clear calm atmosphere and smooth sea, it was less than 3 miles.

These discrepancies were proved to be, in great part, due to a state of the air which bears the same relation to sound that cloudiness does to light. By streams of air differently heated, or saturated in different degrees with aqueous vapour, the atmosphere is rendered *flocculent* to sound.

Acoustic clouds, in fact, are incessantly floating or flying through the air. They have nothing whatever to do with ordinary clouds, fogs, or haze. The most trans-

parent atmosphere may be filled with them ; converting days of extraordinary optical transparency into days of equally extraordinary acoustic opacity.

The connection hitherto supposed to exist between a clear atmosphere and the transmission of sound is therefore dissolved.

The intercepted sound is wasted by repeated reflections in the acoustic cloud, as light is wasted by repeated reflections in an ordinary cloud. And as from the ordinary cloud the light reflected reaches the eye, so from the perfectly invisible acoustic cloud the reflected sound reaches the ear.

Aerial echoes of extraordinary intensity and of long duration may be thus produced. They occur, contrary to the opinion hitherto entertained, in the clearest air.

The existence of these aerial echoes has been proved both by observation and experiment. They may arise either from air-currents differently heated, or from air-currents differently saturated with vapour.

Rain has no sensible power to obstruct sound.

Hail has no sensible power to obstruct sound.

Snow has no sensible power to obstruct sound.

Fog has no sensible power to obstruct sound. p 331

The air associated with fog is, as a general rule, highly homogeneous and favourable to the transmission of sound. The notions hitherto entertained regarding the action of fog are untenable.

Experiments on artificial showers of rain, hail, and snow, and on artificial fogs of extraordinary density, confirm the results of observation.

As long as the air forms a continuous medium, the amount of sound scattered by small bodies suspended in it is astonishingly small.

This is illustrated by the ease with which sound traverses layers of calico, cambric, silk, flannel, baize, and

felt. It freely passes through all these substances in thicknesses sufficient to intercept the light of the sun.

Through six layers of thin silk, for example, it passes with little obstruction ; it finds its way through a layer of close felt half an inch thick, and it is not wholly intercepted by 200 layers of cotton-net.

The atmosphere exercises a selective choice upon the waves of sound which varies from day to day, and even from hour to hour. It is sometimes favourable to the transmission of the longer, and at other times favourable to the transmission of the shorter, sonorous waves.

The recognised action of the wind has been confirmed by this investigation.

This action is explained by the theory of Professor Stokes, which was first verified by the experiments of Professor Osborne Reynolds.

LECTURE VIII.

LAW OF VIBRATORY MOTIONS IN WATER AND AIR—SUPERPOSITION OF VIBRATIONS—INTERFERENCE OF SONOROUS WAVES—DESTRUCTION OF SOUND BY SOUND—COMBINED ACTION OF TWO SOUNDS NEARLY IN UNISON WITH EACH OTHER—THEORY OF BEATS—OPTICAL ILLUSTRATION OF THE PRINCIPLE OF INTERFERENCE—AUGMENTATION OF INTENSITY BY PARTIAL EXTINCTION OF VIBRATIONS—DUANE'S SOUNDLESS ZONES—RESULTANT TONES—CONDITIONS OF THEIR PRODUCTION—EXPERIMENTAL ILLUSTRATIONS—DIFFERENCE TONES AND SUMMATION TONES—THEORIES OF YOUNG AND HELMHOLTZ.

§ 1. *Interference of Water-Waves.*

FROM a boat in Cowes Harbour, in moderate weather, I have often watched the masts and ropes of the ships as mirrored in the water. The images of the ropes revealed the condition of the surface, indicating by long and wide protuberances the passage of the larger rollers, and, by smaller indentations, the ripples which crept like parasites over the sides of the larger waves. The sea was able to accommodate itself to the requirements of all its undulations, great and small. When the surface was touched with an oar, or when drops were permitted to fall from the oar into the water, there was also room for the tiny wavelets thus generated. This carving of the surface by waves and ripples had its limit only in my powers of observation; every wave and every ripple asserted its right of place, and retained its individual existence, amid the crowd of other motions which agitated the water.

The law that rules this chasing of the sea, this crossing and intermingling of innumerable small waves, is *that the*

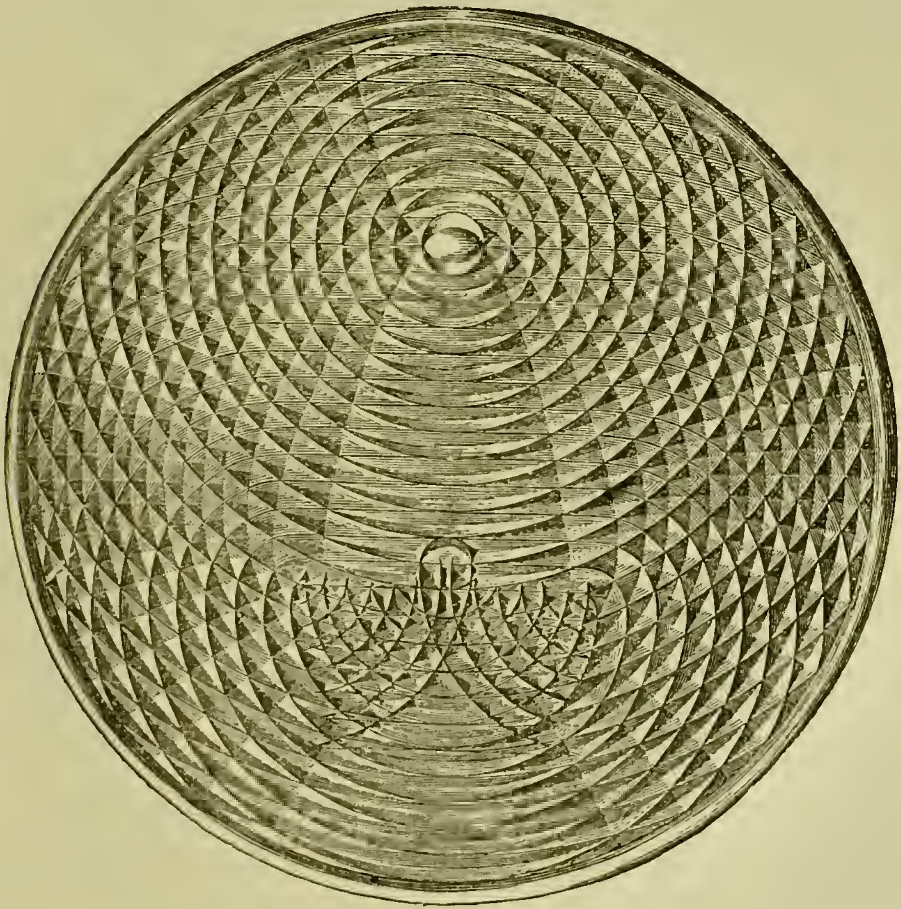
resultant motion of every particle of water is the sum of the individual motions imparted to it. If a particle be acted on at the same moment by two impulses, both of which tend to raise it, it will be lifted by a force equal to the sum of both. If acted upon by two impulses, one of which tends to raise it, and the other to depress it, it will be acted upon by a force equal to the difference of both. When, therefore, the sum of the motions is spoken of, the *algebraic sum* is meant—the motions which tend to raise the particle being regarded as positive, and those which tend to depress it as negative.

When two stones are cast into smooth water, 20 or 30 feet apart, round each stone is formed a series of expanding circular waves, every one of which consists of a ridge and a furrow. The waves touch, cross each other, and carve the surface into little eminences and depressions. Where ridge coincides with ridge, we have the water raised to a double height; where furrow coincides with furrow, we have it depressed to a double depth; where ridge coincides with furrow, we have the water reduced to its average level. The resultant motion of the water at every point is, as above stated, the algebraic sum of the motions impressed upon that point. And if, instead of two sources of disturbance, we had ten, or a hundred, or a thousand, the consequence would be the same; the actual result might transcend our powers of observation, but the law above enunciated would still hold good.

Instead of the intersection of waves from two distinct centres of disturbance, we may cause direct and reflected waves, from the same centre, to cross each other. Many of you know the beauty of the effects produced when light is reflected from ripples of water. When mercury is employed the effect is more brilliant still. Here, by a proper mode of agitation, direct and reflected waves may be caused to cross and interlace, and by the most wonder-

ful self-analysis to untie their knotted scrolls. The adjacent figure (fig. 162), which is copied from the excellent *Wellenlehre* of the brothers Weber, will give some idea of the beauty of these effects. It represents the chasing produced by the intersection of direct and reflected water-waves in a circular vessel, the point of disturbance (marked by the smallest circle in the figure) being midway between the centre and the circumference.

Fig. 162.



This power of water to accept and transmit multitudinous impulses is shared by air, which concedes the right of space and motion to any number of sonorous waves. The same air is competent to accept and transmit the vibrations of a thousand instruments at the same time. When we try to visualise the motion of that air—to present to the eye of the mind the battling of the pulses direct and reverberated—the imagination retires baffled

from the attempt. Still, amid all the complexity, the law above enunciated holds good, every particle of air being animated by a resultant motion, which is the algebraic sum of all the individual motions imparted to it. And the most wonderful thing of all is, that the human ear, though acted on only by a cylinder of that air, which does not exceed the thickness of a quill, can detect the components of the motion, and, by an act of attention, can even isolate from the aerial entanglement any particular sound.

§ 2. *Interference of Sound.*

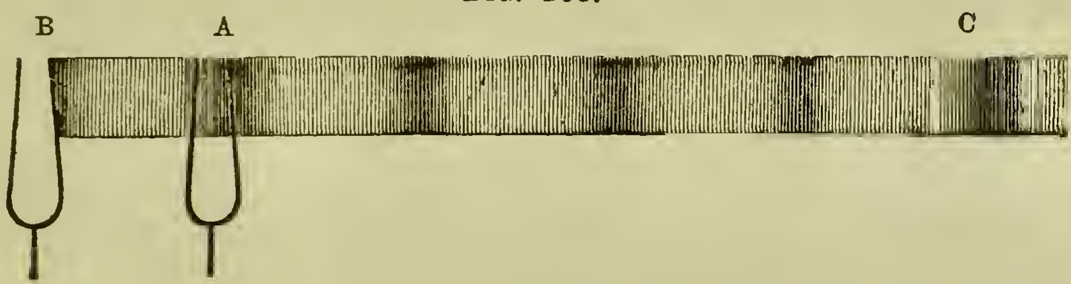
When two unisonant tuning-forks are sounded together, it is easy to see that the forks may so vibrate that the condensations of the one shall coincide with the condensations of the other, and the rarefactions of the one with the rarefactions of the other. If this be the case the two forks will assist each other. The condensations will, in fact, become more condensed, the rarefactions more rarefied; and as it is upon the difference of density between the condensations and rarefactions that *loudness* depends, the two vibrating forks, thus supporting each other, will produce a sound of greater intensity than that of either of them vibrating alone.

It is, however, also easy to see that the two forks may be so related to each other that one of them shall require a condensation at the place where the other requires a rarefaction; that the one fork shall urge the air-particles forward, while the other urges them backward. If the opposing forces be equal, particles so solicited will move neither backwards nor forwards, the aerial rest which corresponds to silence being the result. Thus, it is possible, by adding the sound of one fork to that of another, to abolish the sounds of both. We have here a phenomenon which, above all others, characterises wave-motion. It

was this phenomenon, as manifested in optics, that led to the undulatory theory of light, the most cogent proof of that theory being based upon the fact that, by adding light to light, we may produce darkness, just as we can produce silence by adding sound to sound.

During the vibration of a tuning-fork the distance between the two prongs is alternately increased and dimi-

FIG. 163.

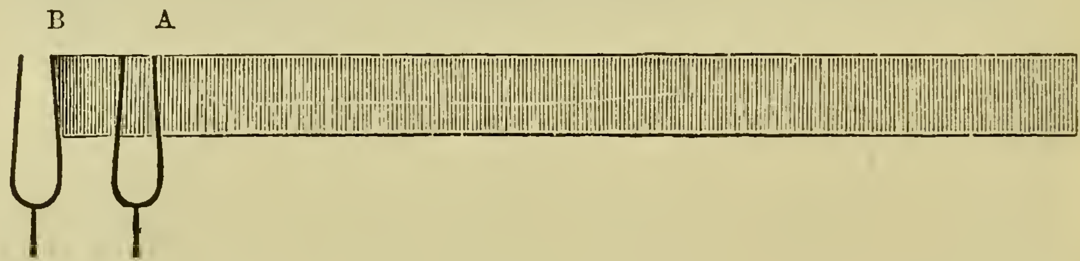


nished. Let us call the motion which increases the distance the *outward swing*, and that which diminishes the distance the *inward swing* of the fork. And let us suppose that our two forks, A and B, fig. 163, reach the limits of their outward swing and their inward swing at the same moment. In this case the *phases* of their motion, to use the technical term, are the same. For the sake of simplicity we will confine our attention to the right-hand prongs, A and B, of the two forks, neglecting the other two prongs; and now let us ask what must be the distance between the prongs A and B, when the condensations and rarefactions of both, indicated respectively by the dark and light shading, coincide? A little reflection will make it clear, that if the distance from B to A be equal to the length of a whole sonorous wave, coincidence between the two systems of waves must follow. The same would evidently occur were the distance between A and B two wave-lengths, three wave-lengths, four wave-lengths—in short, any number of whole wave-lengths. In all such cases we should have coincidence of the two systems of waves, and consequently a reinforcement of the sound of

the one fork by that of the other. Both the condensations and rarefactions between A and C are, in this case, more pronounced than they would be if either of the forks were suppressed.

But if the prong B be only half the length of a wave behind A, what must occur? Manifestly the rarefactions of one of the systems of waves will then coincide with the condensations of the other system, the air to the right of A being reduced to quiescence. This is shown in fig. 164,

FIG. 164.



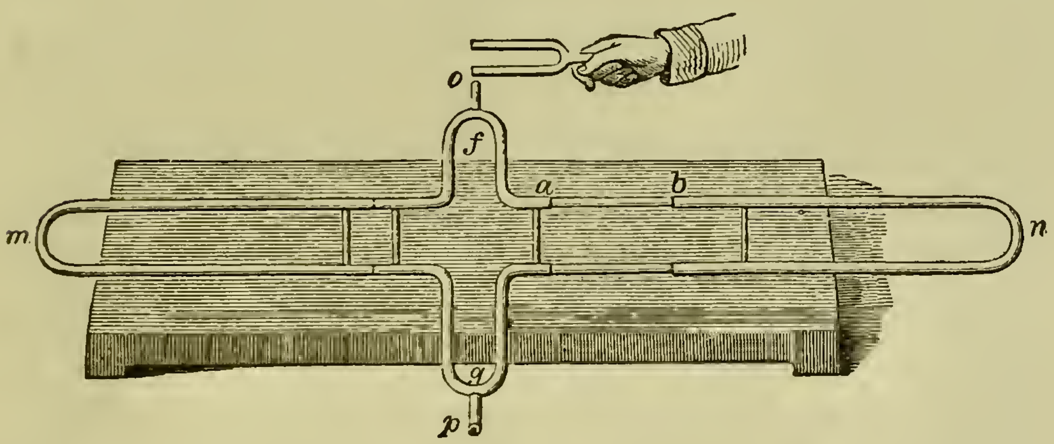
where the uniformity of shading indicates an absence both of condensations and rarefactions. When B is two half wave-lengths behind A, the waves, as already explained, support each other; when they are three half wave-lengths apart, they destroy each other. Or, expressed generally, we have augmentation or destruction according as the distance between the two prongs amounts to an even or an odd number of semi-undulations. Precisely the same is true of the waves of light. If through any cause one system of ethereal waves be any *even* number of semi-undulations behind another system, the two systems support each other when they coalesce, and we have more light. If the one system be any *odd* number of semi-undulations behind the other, they oppose each other, and a destruction of light is the result of their coalescence.

The action here referred to, both as regards sound and light, is called *Interference*.

§ 3. *Experimental Illustrations.*

Sir John Herschel was the first to propose to divide a stream of sound into two branches, of different lengths, causing the branches afterwards to reunite, and interfere with each other. This idea has been recently followed out with success by M. Quincke; and it has been still further improved upon by M. König. The principle of these experiments will be at once evident from fig. 165. The

FIG. 165.



tube $o f$ divides into two branches at f, the one branch being carried round n, and the other round m. The two branches are caused to reunite at g, and to end in a common canal, $g p$. The portion $b n$ of the tube slides over $a b$, and can be drawn out as shown in the figure; thus the sound-waves can be caused to pass over different distances in the two branches. Placing a vibrating tuning-fork at o, and the ear at p, when the two branches are of the same length, the waves through both reach the ear together, and the sound of the fork is heard. Drawing $n b$ out, a point is at length obtained where the sound of the fork is extinguished. This occurs when the distance $a b$ is one-fourth of a wave-length; or, in other words, when the whole right-hand branch is half a wave-length longer than the left-hand one. Drawing $b n$ still further out, the sound is again heard; and when twice

the distance *a b* amounts to a whole wave-length, it reaches a maximum. Thus according as the difference of both branches amounts to half a wave-length, or to a whole wave-length, we have reinforcement or destruction of the two series of sonorous waves. In practice the tube *o f* ought to be prolonged until the direct sound of the fork is unheard, the attention of the ear being then wholly concentrated on the sounds that reach it through the tube.

It is quite plain that the wave-length of any simple tone may be readily found by this instrument. It is only necessary to ascertain the difference of path which produces complete interference. Twice this difference is the wave-length; and if the rate of vibration be at the same time known, we can immediately calculate the velocity of sound in air.

Each of the two forks now before you executes exactly 256 vibrations in a second. Sounded together, they are in unison. Loading one of them with a bit of wax, it vibrates a little more slowly than its neighbour. The wax, say, reduces the number of vibrations to 255 in a second: how must their waves affect each other? If they start at the same moment, condensation coinciding with condensation, and rarefaction with rarefaction, it is quite manifest that this state of things cannot continue. At the 128th vibration their phases are in complete opposition, one of them having gained half a vibration on the other. Here the one fork generates a condensation where the other generates a rarefaction: and the consequence is, that the two forks, at this particular point, completely neutralise each other. From this point onwards, however, the forks support each other more and more, until, at the end of a second, when the one has completed its 255th, and the other its 256th vibration, condensation again coincides with condensation, and rarefaction with rarefaction, the full effect of both sounds being produced upon the ear.

It is quite manifest that under these circumstances we cannot have the continuous flow of perfect unison. We have, on the contrary, an alternate rising and falling of the sound. We obtain, in fact, the effect known to musicians by the name of *beats*, which, as here explained, are a result of interference.

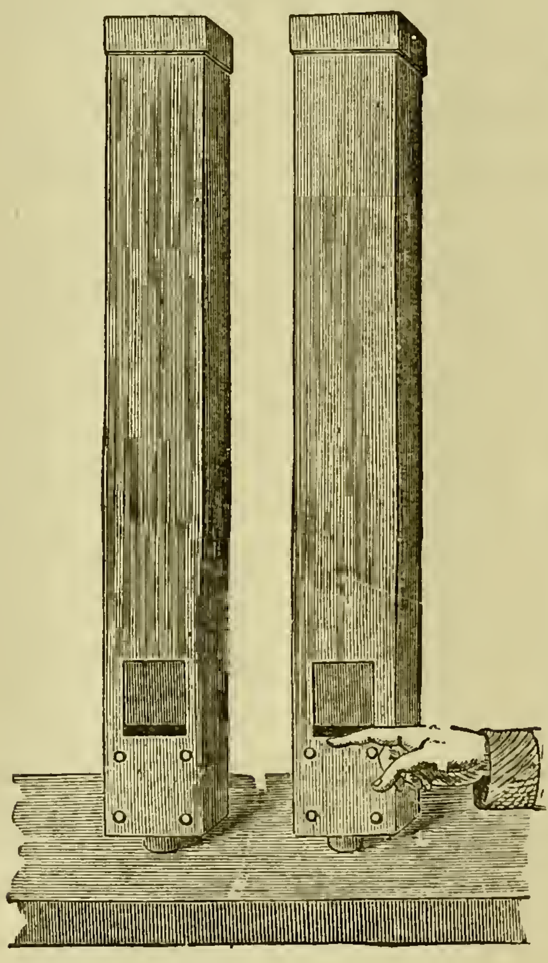
I now load this fork still more heavily, by attaching a fourpenny-piece to the wax; the coincidences and interferences follow each other more rapidly than before; we have a quicker succession of beats. In our last experiment, the one fork accomplished one vibration more than the other in a second, and we had a single beat in the same time. In the present case, one fork vibrates 250 times, while the other vibrates 256 times in a second, and the number of beats per second is 6. A little reflection will make it plain that in the interval required by the one fork to execute one vibration more than the other a beat must occur; and inasmuch as, in the case now before us, there are six such intervals in a second, there must be six beats in the same time. In short, *the number of beats per second is always equal to the difference between the two rates of vibration.*

§ 4. *Interference of Waves from Organ-pipes.*

Beats may be produced by all sonorous bodies. These two tall organ-pipes, for example, when sounded together, give powerful beats, one of them being slightly longer than the other. Here are two other pipes, which are now in perfect unison, being exactly of the same length. But it is only necessary to bring the finger near the embouchure of one of the pipes, fig. 166, to lower its rate of vibration, and produce loud and rapid beats. The placing of the hand over the open top of one of the pipes also lowers its rate of vibration, and produces beats, which

follow each other with augmented rapidity as the top of the pipe is closed more and more. By a stronger blast the two first harmonics of the pipes are brought out. These higher notes also interfere, and you have these quicker beats.

Fig. 166.

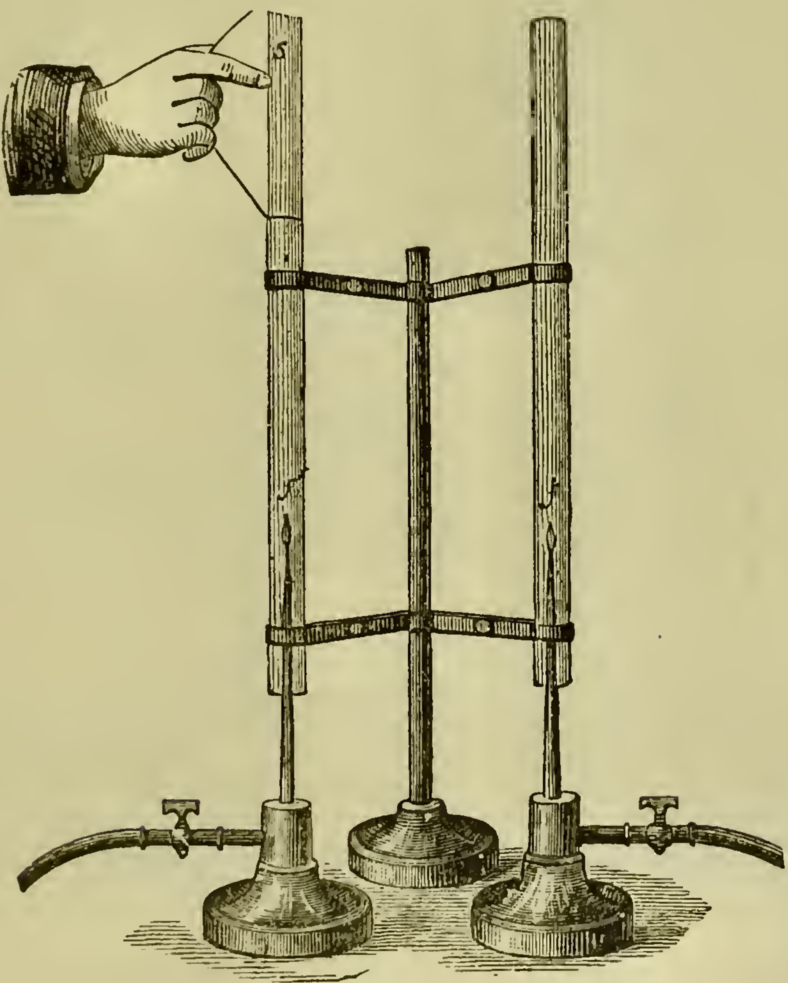


No more beautiful illustration of this phenomenon can be adduced than that furnished by two sounding flames. Two such flames are now before you, the tube surrounding one of them being provided with a telescopic slider, fig. 167 (next page) There are at present no beats, because the tubes are not sufficiently near unison. I gradually lengthen the shorter tube by raising its slider. Rapid beats are now heard ; now they are slower ; now slower still ; and now both flames sing together in perfect unison. Continuing the upward motion of the slider, I make the tube too long ; the beats begin again, and quicken, until finally their sequence is so rapid as to appeal only as roughness to the ear. The flames, you observe, dance within their tubes in time to the beats. As already stated, these beats cause a silent flame within a tube to quiver when the voice is thrown to a proper pitch, and when the position of the flame is rightly chosen, the beats set it singing. With the flames of large roseburners, and with tin tubes from 3 to 9 feet long, we obtain beats of exceeding power.

You have just heard the beats produced by two tall

organ-pipes nearly in unison with each other. Two other pipes are now mounted on our wind-chest, fig. 168 (next page) each of which, however, is provided at its centre with a membrane intended to act upon a flame.[1] Two small tubes lead from the spaces closed by the membranes, and unite

Fig. 167.

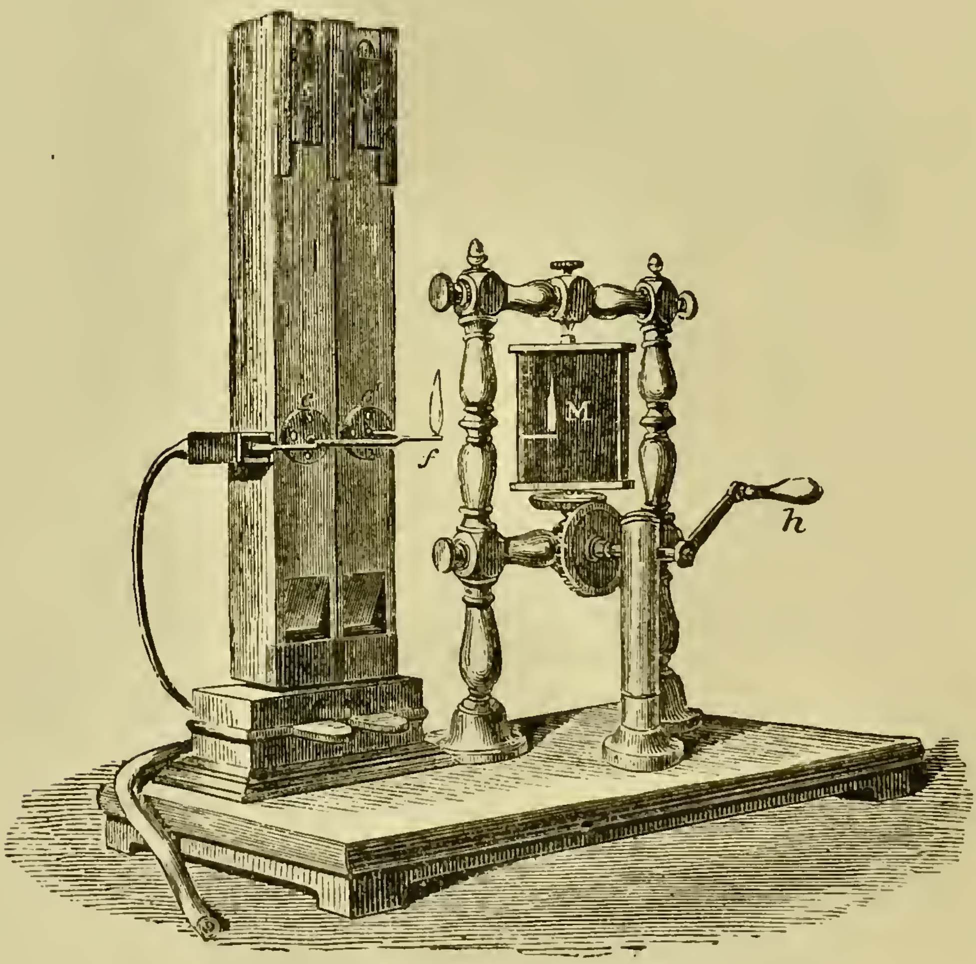


afterwards, the membranes of both the organ-pipes being teus connected with the same flame. By means of the sliders, *s*, *s*′, near the summits of the pipes, they are either brought into unison or thrown out of it at pleasure. They are not at present in unison, and the beats they produce follow each other with great rapidity. The flame connected with the central membranes dances in time to the beats. When brought nearer to unison, the beats are

[1] Described in Lecture V.

slower, and the flame at successive intervals withdraws its light and exhales it. A process which reminds you of the inspiration and expiration of the breath is thus carried on by the flame. If the mirror, M, be now turned, the flame produces a luminous band—continuous at cer-

FIG. 168.



tain places, but for the most part broken into distinct images of the flame. The continuous parts correspond to the intervals of interference where the two sets of vibrations abolish each other.

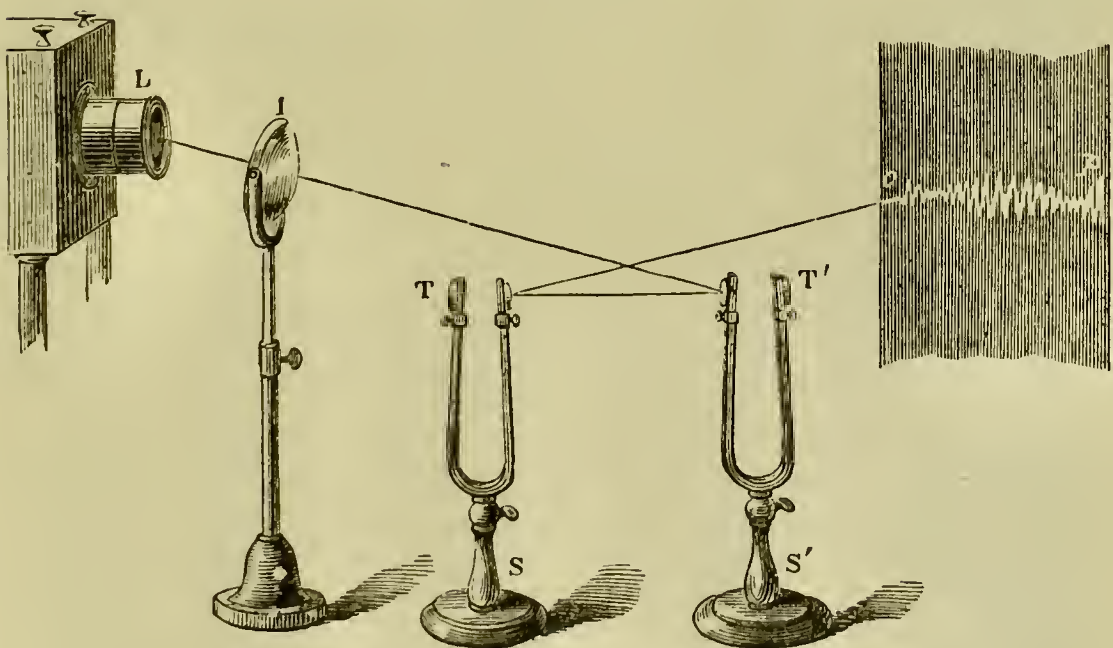
Instead of permitting both pipes to act upon the same flame, we may associate a flame with each of them. The deportment of the flames is then very instructive. Imagine both flames to be in the same vertical line, the one of them being exactly under the other. Bringing the pipes into unison, and turning the mirror, we resolve each

flame into a chain of images, but we notice that the images of the one occupy the spaces *between* the images of the other. The periods of extinction of the one flame, therefore, correspond to the periods of kindling of the other. The experiment proves that when two unisonant pipes are placed thus close to each other, their vibrations are in opposite phases. The consequence of this is, that the two sets of vibrations permanently neutralise each other, so that at a little distance from the pipes you fail to hear the fundamental tone of either. For this reason we cannot, with any advantage, place close to each other in an organ several pipes of the same pitch.

§ 5. *Lissajous' Illustration of the Beats of two Tuning-forks.*

In the case of beats, the amplitude of the oscillating air reaches a maximum and a minimum periodically. By

FIG. 169.



the beautiful method of M. Lissajous we can illustrate optically this alternate augmentation and diminution of amplitude. Placing a large tuning-fork, T′, fig 169, in front of the lamp, L, a luminous beam is received upon

the mirror attached to the fork. This is reflected back to the mirror of a second fork, T, and by it thrown on to the screen, where it forms a luminous disc. When the bow is drawn over the fork T′, the beam, as in the experiments described in the second Lecture, is tilted up and down, the disc upon the screen stretching to a luminous band two or three feet long. If, in drawing the bow over this second fork, the vibrations of both coincide in phase, the band will be lengthened; if the phases are in opposition, total or partial neutralisation of the one fork by the other will be the result. It so happens that in the present instance the second fork adds something to the action of the first, the band of light being now four feet long. These forks have been tuned as perfectly as possible. Each of them executes exactly 64 vibrations in a second; the initial relation of their phases remains, therefore, constant, and hence you notice a gradual shortening of the luminous band, like that observed during the subsidence of the vibration of a single fork. The band at length dwindles to the original disc, which remains motionless upon the screen.

By attaching, with wax, a threepenny-piece to the prong of one of these forks, its rate of vibration is lowered. The phases of the two forks cannot now retain a constant relation to each other. One fork incessantly gains upon the other, and the consequence is that sometimes the phases of both coincide, and at other times they are in opposition. Observe the result. At the present moment the two forks conspire, and we have a luminous band four feet long upon the screen. This slowly contracts, drawing itself up to a mere disc; but the action halts here only during the moment of opposition. That passed, the forks begin again to assist each other, and the disc once more slowly stretches into a band. The action here is very slow; but it may be quickened by attaching a sixpence

to the loaded fork. The band of light now stretches, and contracts in perfect rhythm. The action, rendered thus optically evident, is impressed upon the air of this room ; its particles alternately vibrate and come to rest, and, as a consequence, beats are heard in synchronism with the changes of the figure upon the screen.

The time which elapses from maximum to maximum, or from minimum to minimum, is that required for the one fork to perform one vibration more than the other. At present this time is about two seconds. In two seconds, therefore, one beat occurs. When we augment the dissonance by increasing the load, the rhythmic lengthening and shortening of the band is more rapid, while the intermittent hum of the forks is more audible. There are now six elongations and shortenings in the interval taken up a moment ago by one; the beats at the same time being heard at the rate of three a second. By loading the fork still more, the alternations may be caused to succeed each other so rapidly that they can no longer be followed by the eye, while the beats, at the same time, cease to be individually distinct, and appeal as a kind of roughness to the ear.

In the experiments with a single tuning-fork, already described (fig. 22, Lecture II.), the beam reflected from the fork was received on a looking-glass, and, by turning the glass, the band of light on the screen was caused to stretch out into a long wavy line. It was explained at the time that the loudness of the sound depended on the depth of the indentations. Hence, if the band of light of varying length now before us on the screen be drawn out in a sinuous line, the indentations ought to be at some places deep, while at others they ought to vanish altogether. This is the case. By a little tact the mirror of the fork T (fig. 169) is caused to turn through a small angle, a sinuous line composed of swellings and contractions (fig.

170) being drawn upon the screen. The swellings correspond to the periods of sound, and the contractions to those of silence.[1]

Two vibrating bodies, then, each of which separately produces a musical sound, can, when acting together, neutralise each other. Hence, by quenching the vibrations of one of them, we may give sonorous effect to the other. It often happens, for instance, that when two tuning-forks, on their resonant cases, are vibrating in unison, the stoppage of one of them is accompanied by an augmentation of the sound. This point may be further illustrated by the vibrating bell, already described (fig. 78, Lecture IV.). Placing its resonant tube in front of one of its nodes, a sound is heard, but nothing like what is heard when the tube is opposed to a ventral segment. The reason of this is that the vibrations of a bell on the opposite sides of a nodal line are in opposite directions, and they therefore interfere with each other. By introducing a glass plate between the bell and the tube, the vibrations on one side of the nodal line may be intercepted; an instant augmentation of the sound is the consequence.

FIG. 171.

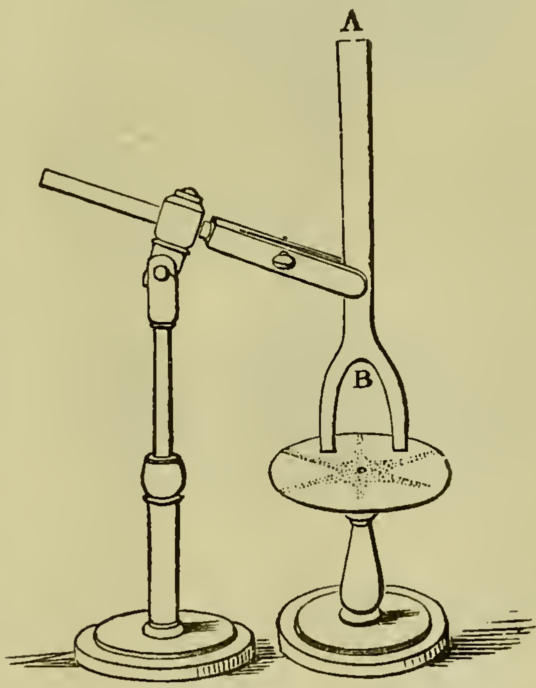
§ 6. *Interference of Waves from a Vibrating Disc. Hopkins' and Lissajous' Illustrations.*

In a vibrating disc every two adjacent sectors move at the same time in opposite directions. When the one sector rises the other falls, the nodal line marking the

[1] The figure is but a meagre representation of the fact. The band of light was two inches wide, the depth of the sinuosities varying from three feet to zero.

limit between them. Hence, at the moment when any sector produces a condensation in the air above it, the adjacent sector produces a rarefaction in the same air. A partial destruction of the sound of one sector by the other is the result. You will now understand the instrument by which the late William Hopkins illustrated the principle of interference. The tube A B, fig. 171, divides at B into two branches. The end A of the tube is closed by a membrane. Scattering sand upon this membrane, and holding the ends of the branches over *adjacent* sectors of a vibrating disc, no motion (or at least an extremely feeble motion) of the sand is perceived. Placing the ends of the two branches over *alternate* sectors of the disc, the sand is tossed from the membrane, proving that in this case we have coincidence of vibration on the part of the two sectors.

Fig. 171.



We are now prepared for a very instructive experiment, which we owe to M. Lissajous. Drawing a bow over the edge of a brass disc, I divide it into six vibrating sectors. When the palm of the hand is brought over any one of them, the sound, instead of being diminished, is augmented. When two hands are placed over two *adjacent* sectors, you notice no increase of the sound; but when they are placed over *alternate* sectors, as in fig. 172, a striking augmentation of the sound is the consequence. By simply lowering and raising the hands, marked variations of intensity are produced. By the

approach of the hands the vibrations of the two sectors are intercepted; their interference right and left being thus abolished, the remaining sectors sound more loudly. Passing the single hand to and fro over the surface, you also hear a rise and fall of the sound. It rises when the hand is over a vibrating sector; it falls when the hand is over a nodal line. Thus, by sacrificing a portion of the vibrations, we make the residue more effectual. Experiments similar to these may be made with light and radiant heat. If of two beams of the former, which destroy each other by interference, one be removed, light takes the place of darkness; and if of two interfering beams of the latter one be intercepted, heat takes the place of cold.

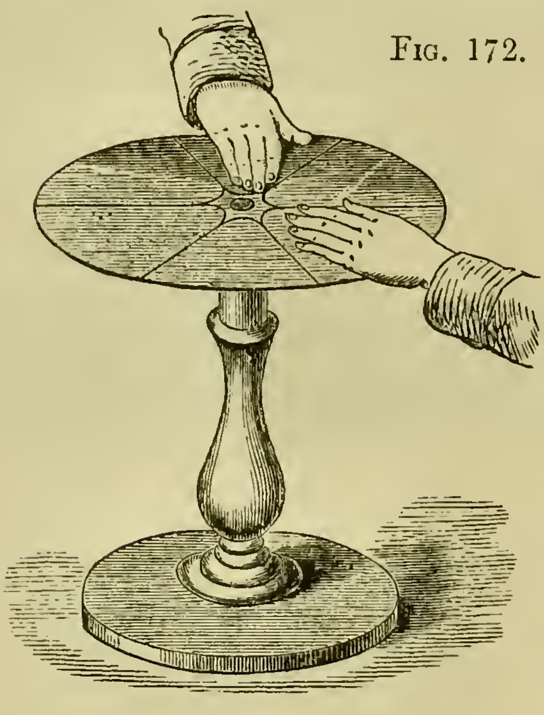


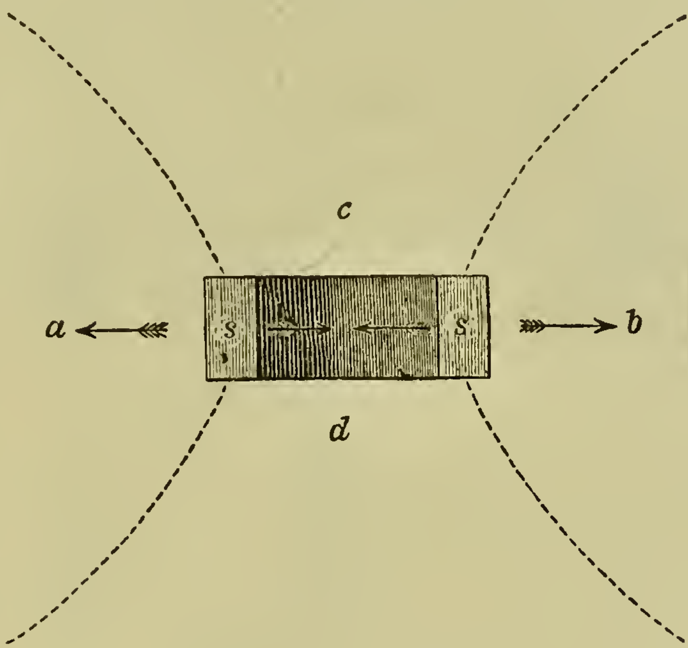
Fig. 172.

§ 7. *Quenching the Sound of one Prong of a Tuning-fork by that of the other.*

You have remarked the almost total absence of sound on the part of a vibrating tuning-fork when held free in the hand. The feebleness of the fork as a sounding body arises in part from interference. The prongs always vibrate in opposite directions, one producing a condensation where the other produces a rarefaction, a destruction of sound being the consequence. By simply passing a pasteboard tube over one of the prongs of the fork, its vibrations are in part intercepted, and an augmentation of the sound is the result. The single prong is thus proved to be more effectual than the two prongs. There are positions in

which the destruction of the sound of one prong by that of the other is *total*. These positions are easily found by striking the fork and turning it round close to the ear. When the back of the prong is parallel to the ear, the sound is heard; when the side surfaces of both prongs are parallel to the ear, the sound is also heard; but when the *corner* of a prong is carefully presented to the ear the sound is utterly destroyed. During one complete rotation of the fork we find four positions where the sound is thus obliterated.

FIG. 173.

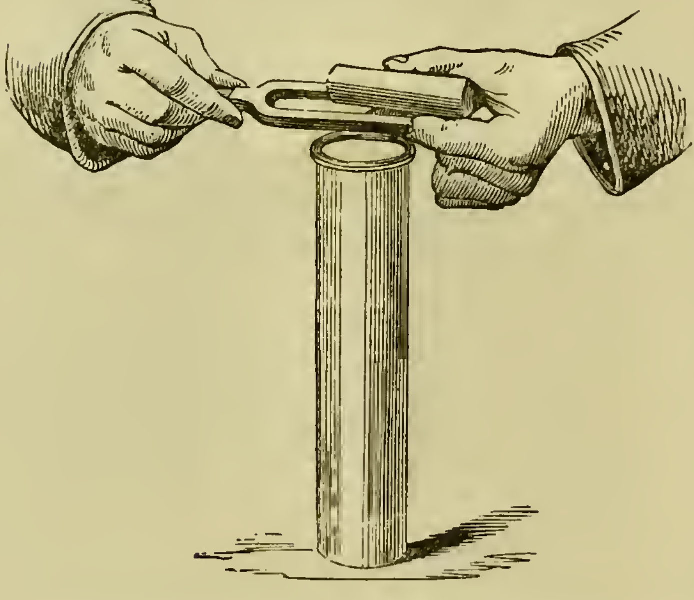


Let *s s* (fig. 173) represent the two ends of the tuning-fork, looked-down-upon as it stands upright. When the ear is placed at *a* or *b*, or at *c* or *d*, the sound is heard. Along the four dotted lines, on the contrary, the waves generated by the two prongs completely neutralise each other, and nothing is there heard. These lines have been proved by Weber to be hyperbolic curves; and this must be their character according to the principle of interference.

This remarkable case of interference, which was first noticed by Dr. Thomas Young, and thoroughly investigated by the brothers Weber, may be rendered audible by means of resonance. Bringing a vibrating fork over a jar which resounds to it, and causing the fork to rotate slowly, in four positions we have a loud resonance; in four others absolute silence, alternate risings and fall-

ings of the sound accompanying the fork's rotation. While the fork is over the jar with its corner downwards, and the sound entirely extinguished, let a pasteboard tube be passed over one of its prongs, as in fig. 174, a loud resonance announces the withdrawal of the vibrations of that prong. To obtain this effect, the fork must be held over the centre of the jar, so that the air shall be symmetrically distributed on both sides of it. Moving the fork from the centre towards one of the sides, without altering its inclination in the least, we obtain a forcible sound. Interference, however, is also possible near the side of the jar. Holding the fork, not with its corner downwards, but with both its prongs in the same horizontal plane, a position is soon found near the side of the jar where the sound is extinguished. In passing completely from side to side over the mouth of the jar, two such places of interference are discoverable.

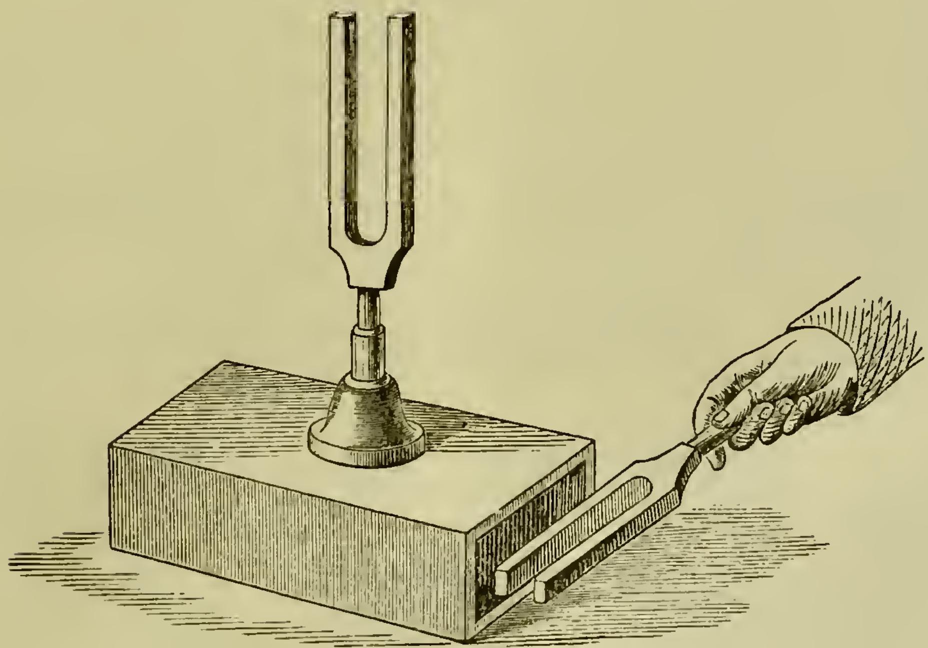
FIG. 174.



A variety of experiments will suggest themselves to the reflecting mind, by which the effect of interference may be illustrated. It is easy, for example, to find a jar which resounds to a vibrating plate. Such a jar, placed over a vibrating segment of the plate, produces a powerful resonance. Placed over a nodal line, the resonance is entirely absent; but if a piece of pasteboard be interposed between the jar and plate, so as to cut off the vibrations on one side of the nodal line, the jar instantly resounds

to the vibrations of the other. Again, holding two forks, which vibrate with the same rapidity, over two resonant jars, the sound of both flows forth in unison. When a bit of wax is attached to one of the forks, powerful beats are heard. Removing the wax, the unison is restored. When one of these unisonant forks is placed in the flame of a spirit-lamp its elasticity is changed, and it produces long loud beats with its unwarmed fellow.[1] If while one of the forks is sounding on its resonant case, the other be excited and brought near the mouth of the case, as in fig.

FIG. 175.



175, loud beats declare the absence of unison. Dividing a jar by a vertical diaphragm, and bringing one of the forks over one of its halves, and the other fork over the other; the two semi-cylinders of air produce beats by their interference. But the diaphragm is not necessary; on removing it, the beats continue as before, one half of the same column of air interfering with the other [2]

[1] In his admirable experiments on tuning, Scheibler found in the beats a test of differences of temperature of exceeding delicacy.

[2] Sir John Herschel and Sir C. Wheatstone, I believe, made this experiment independently.

The intermittent sound of certain bells, heard more especially when their tones are subsiding, is an effect of interference. The bell, through lack of symmetry, as explained in the fourth Lecture, vibrates in one direction a little more rapidly than in the other, and beats are the consequence of the coalescence of the two different rates of vibration.

49 & 150

§ 8. *Soundless Zones of General Duane.*

It gives me special pleasure to introduce here an account of some observations by General Duane upon the fog signals on the coast of Maine, in the United States:—'There are,' he says, 'six steam fog-whistles on the coast of Maine. These have been frequently heard at a distance of twenty miles, and as frequently cannot be heard at the distance of two miles, and this with no perceptible difference in the state of the atmosphere.' This entirely agrees with the observations at the South Foreland.

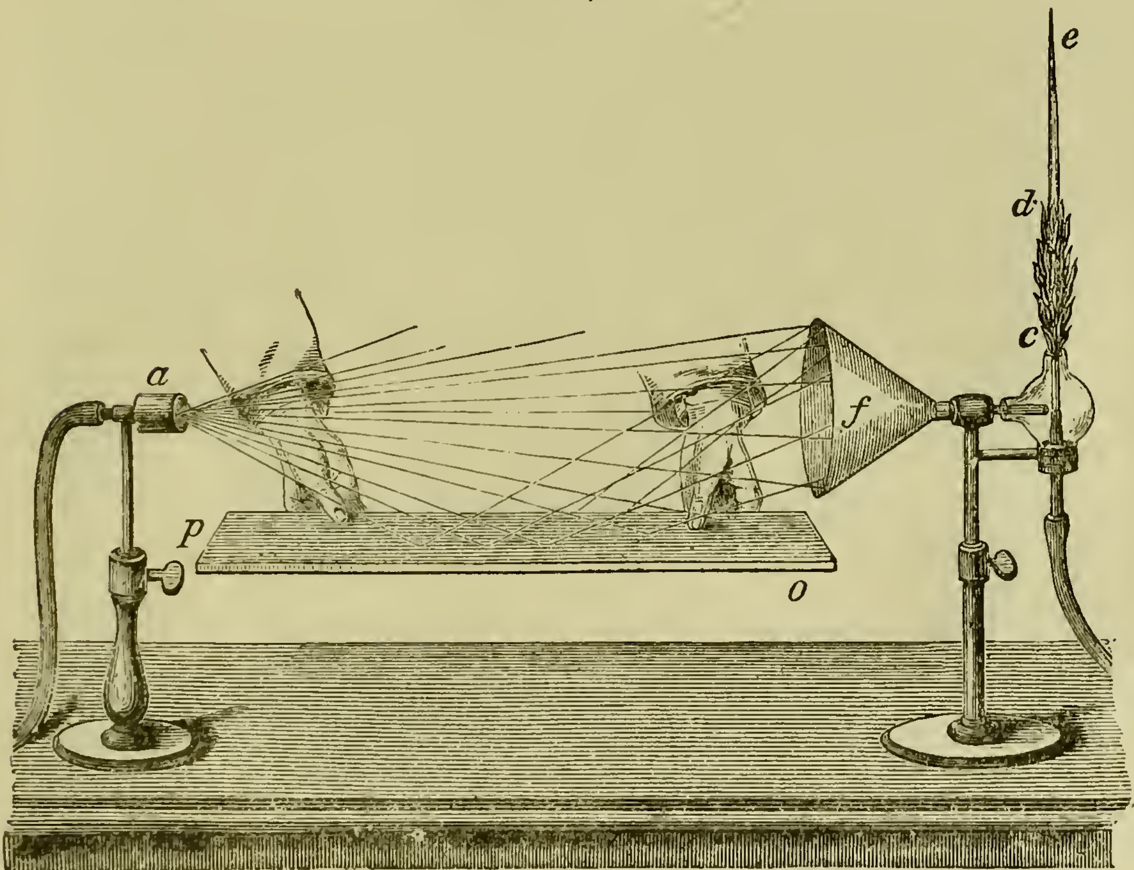
'The signal,' continues General Duane, 'is often heard at a great distance in one direction, while in another it would be scarcely audible at the distance of a mile. This is not the effect of wind, as the signal is frequently heard much further against the wind than with it. For example, the whistle on Cape Elizabeth can always be heard distinctly in Portland, a distance of nine miles, during a heavy north-east snow-storm, the wind blowing a gale directly from Portland towards the whistle.'

This also is in strict agreement with the observations at the South Foreland.

General Duane next mentions a fact of singular interest and significance. 'The most perplexing difficulties,' he says, 'arise from the fact that the signal often appears to be surrounded by a belt varying in radius from 1 to $1\frac{1}{2}$

miles from which the sound appears to be entirely absent. Thus, in moving directly from a station the sound is audible for the distance of a mile, is then lost for about the same distance, after which it is again distinctly heard for a long time. This action is common to all ear signals, and has been at times observed at all the stations, at one of which the signal is situated on a bare rock 20 miles

FIG. 176.



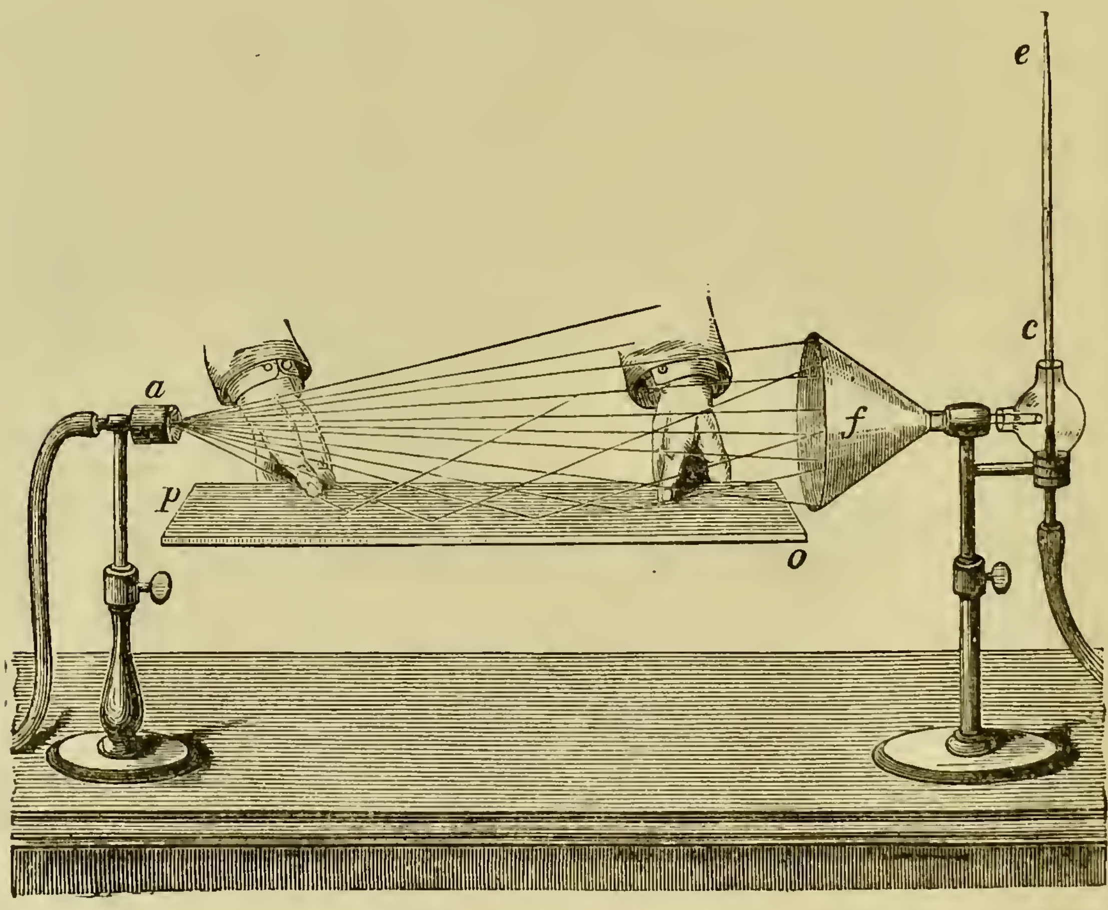
from the mainland, with no surrounding objects to affect the sound.'

General Duane is the only person, as far as I am aware, who has noticed this singular effect, and the thoroughness with which he has followed it up, strikingly demonstrates his keenness and sagacity as an observer. He has offered no explanation of the phenomenon, and we are now, I think, in a condition to give one.

For a long time I have thought that these soundless zones were caused by the interference of the waves coming

directly from the fog signal with waves reflected from the surface of the sea. Our invaluable sensitive flame enables us to test this theory. Observe, in the first place, the influence of reflection in augmenting the action upon the flame. Placing the sounding reed at *a*, fig. 176, and the flame at *c*, we adjust the pressure so as to bring the flame, while burning steadily, near its flaring point. Be-

FIG. 177.



tween *a* and *c* I now introduce a smooth deal board *p o*. By the addition of the waves reflected from the board to the direct waves, the flame is violently agitated. (In the figure the steady flame is shown in combination with the agitated flame.) When the board is removed the flame becomes still, whereas its introduction is always followed by flaring. While the flame is agitated, I throw a piece of coarse woollen cloth over the board. The reflection is thereby so far abolished that the flame is stilled. The removal of the cloth at once restores the agitation.

Here the direct and reflected waves reinforce each other. But it is easy to see that the board may be so placed that the reflected waves, having to pass over a greater distance, will lag by half a wave-length behind the direct ones. If this occur, we should be led on *à priori* grounds to infer that the direct and reflected waves would mutually destroy each other. The experiment requires care, but, if properly made, it is infallibly successful. I first of all so arrange the pressure that, when the reed *a*, fig. 177, sounds, the flame flares. I now carefully lift the smooth deal board between reed and flame; at a certain height the agitation of the flame is increased. Raising the board still higher, the flame *c e* is completely stilled. Thus, by pouring the reflected waves, with a proper amount of retardation, among the direct waves, we sensibly destroy the sound. Instead of our reed, let us imagine a fog-whistle on the coast of Maine; instead of our flame, an observer on the deck of a vessel; and instead of our board, the smooth sea. At a distance from the shore which would cause the reflected waves to be half a wave-length behind the direct ones, we should have the precise conditions illustrated by the experiment just made before you. The union of the direct and reflected waves would thus produce the soundless zone of General Duane.

RESULTANT TONES.

We have now to turn from this question of interference to the consideration of a new class of musical sounds, of which the beats were long considered to be the progenitors. The sounds here referred to require for their production the union of two distinct musical tones. Where such union is effected, under the proper conditions, *resultant tones* are generated, which are quite

distinct from the primaries concerned in their production. They were discovered in 1745, by a German organist named Sorge, but the publication of the fact attracted little attention. They were discovered independently in 1754 by the celebrated Italian violinist Tartini, and after him have been called Tartini's tones.

To produce them it is desirable, if not necessary, to have the two primary tones of considerable intensity Helmholtz prefers the syren to all other means of exciting them, and with this instrument they are very readily obtained. It requires some attention at first, on the part of the listener, to single out the resultant tone from the general mass of sound; but, with a little practice, this is readily accomplished; and though the unpractised ear may fail, in the first instance, thus to analyse the sound, the clang-tint is influenced in an unmistakable manner by the admixture of resultant tones. I set Dove's syren in rotation, and open two series of holes at the same time; with the utmost strain of attention, I am as yet unable to hear the least symptom of a resultant tone. Urging the instrument to greater rapidity, a dull low droning mingles with the two primary sounds. Raising the speed of rotation, the low resultant tone rises rapidly in pitch, and now, to those who stand close to the instrument, it is very audible. The two series of holes here open, number 8 and 12 respectively. The resultant tone is in this case an octave below the deepest of the two primaries. Opening two other series of orifices, numbering 12 and 16 respectively, the resultant tone is quite audible. Its rate of vibration is one-third of the rate of the deepest of the two primaries. In all cases, *the resultant tone is that which corresponds to a rate of vibration equal to the difference of the rates of the two primaries.*

The resultant tone here spoken of is that actually heard in the experiment. But with finer methods of

experiment other resultant tones are proved to exist. Those on which we have now fixed our attention are, however, the most important. They are called *difference tones* by Helmholtz, in consequence of the law just mentioned.

To bring these resultant tones audibly forth, the primaries must, as already stated, be forcible. When they are feeble the resultants are unheard. I am acquainted with no method of exciting these tones more simple and effectual than a pair of suitable singing flames. Two such flames may be caused to emit powerful notes—self-created, self-sustained, and requiring no muscular effort on the part of the observer to keep them going. Here are two of them. The length of the shorter of the two tubes surrounding these flames is $10\frac{3}{8}$ inches, that of the other is $11\frac{2}{5}$ inches. I hearken to the sound, and in the midst of the shrillness detect a very deep resultant tone. The reason of its depth is manifest: the two tubes being so nearly alike in length, the difference between their vibrations is small, and the note corresponding to this difference, therefore, low in pitch. Lengthening one of the tubes by means of its slider, the resultant tone rises gradually in pitch, and at length swells surprisingly. When the tube is shortened the resultant tone falls, and thus by alternately raising and lowering the slider, the resultant tone is caused to rise and sink, in accordance with the law which makes the number of its vibrations the difference between the numbers of its two primaries.

We can determine, with ease, the actual number of vibrations corresponding to any one of those resultant tones. The sound of the flame is that of the open tube which surrounds it, and we have already learned (Lecture III.) that the length of such a tube is half that of the sonorous wave it produces. The wave-length, therefore, corresponding to our $10\frac{3}{8}$-inch tube is $20\frac{3}{4}$ inches. The velocity of sound in air of the present temperature is

1,120 feet a second. Bringing these feet to inches, and dividing by $20\frac{3}{4}$, we find the number of vibrations corresponding to a length of $10\frac{3}{8}$ inches to be 648 per second.

But it must not be forgotten here, that the air in which the vibrations are actually executed is much more elastic than the surrounding air. The flame heats the air of the tube, and the vibrations must, therefore, be executed more rapidly than they would be in an ordinary organ-pipe of the same length. To determine the actual number of vibrations, we must fall back upon our syren; and with this instrument it is found that the air within the $10\frac{3}{8}$-inch tube executes 717 vibrations in a second. The difference of 69 vibrations a second is due to the heating of the aerial column. Carbonic acid and aqueous vapour are, moreover, the product of the flame's combustion, and their presence must also affect the rapidity of the vibration.

Determining in the same way the rate of vibration of the $11\frac{2}{5}$-inch tube, we find it to be 667 per second; the difference between this number and 717 is 50, which expresses the rate of vibration corresponding to the first deep resultant tone.

But this number does not mark the limit of audibility. Permitting the $11\frac{2}{5}$-inch tube to remain as before, and lengthening its neighbour, the resultant tone sinks near the limit of hearing. When the shorter tube measures 11 inches, the deep sound of the resultant tone is still heard. The number of vibrations per second executed in this 11-inch tube is 700. We have already found the number executed in the $11\frac{2}{5}$-inch tube to be 667; hence $700-667=33$, which is the number of vibrations corresponding to the resultant tone now plainly heard when the attention is converged upon it. We here come very near the limit which Helmholtz has fixed as that of musical

audibility. Combining the sound of a tube $17\frac{5}{8}$ inches in length with that of a $10\frac{3}{8}$-inch tube, we obtain a resultant tone of higher pitch than any previously heard. Now, the actual number of vibrations executed in the longer tube is 459; and we have already found the vibrations of our $10\frac{3}{8}$-inch tube to be 717; hence $717-459=258$, which is the number corresponding to the resultant tone now audible. This note is almost exactly that of one of our series of tuning-forks, which vibrates 256 times in a second.

And now we will avail ourselves of a very striking check which this result suggests to us. The well-known fork which vibrates at the rate just mentioned is here, mounted on its case, and I touch it with the bow so lightly that the sound alone could hardly be heard; but it instantly coalesces with the resultant tone, and the beats produced by their combination are clearly audible. By loading the fork, and thus altering its pitch, or by drawing up the paper slider, and thus altering the pitch of the flame, the rate of these beats can be altered, exactly as when we compare two primary tones together. By slightly varying the size of the flame the same effect is produced. We cannot fail to observe how beautifully these results harmonise with each other.

Standing midway between the syren and a shrill singing flame, and gradually raising the pitch of the syren, the resultant tone soon makes itself heard, sometimes swelling out with extraordinary power. When a pitch-pipe is blown near the flame, the resultant tone is also heard, seeming, in this case, to originate in the ear itself, or rather in the brain. By gradually drawing out the stopper of the pipe, the pitch of the resultant tone is caused to vary in accordance with the law already enunciated.

The resultant tones produced by the combination of

the ordinary harmonic intervals [1] are given in the following table:—

Interval	Ratio of vibrations	Difference	The resultant tone is deeper than the lowest primary tone by
Octave . . .	1 : 2	1	0
Fifth . . .	2 : 3	1	an octave
Fourth . . .	3 : 4	1	a twelfth
Major third . .	4 : 5	1	two octaves
Minor third . .	5 : 6	1	two octaves and a major third
Major sixth . .	3 : 5	2	a fifth
Minor sixth . .	5 : 8	3	major sixth

The celebrated Thomas Young thought that these resultant tones were due to the coalescence of rapid beats, which linked themselves together like the periodic impulses of an ordinary musical note. This explanation harmonised with the fact that the number of the beats, like that of the vibrations of the resultant tone, is equal to the difference between the two sets of vibrations. This explanation, however, is insufficient. The beats tell more forcibly upon the ear than any continuous sound. They can be plainly heard when each of the two sounds that produce them has ceased to be audible. This depends in part upon the sense of hearing, but it also depends upon the fact that when two notes of the same intensity produce beats, the amplitude of the vibrating air-particles is at times destroyed, and at times doubled. But by doubling the amplitude we quadruple the intensity of the sound. Hence when two notes of the same intensity produce beats, *the sound incessantly varies between silence and a tone of four times the intensity of either of the interfering ones.*

If, therefore, the resultant tones were due to the beats of their primaries, they ought to be heard, even when the

[1] A subject to be dealt with in Lecture IX.

primaries are feeble. But they are not heard under these circumstances. When several sounds traverse the same air, each particular sound passes through the air as if it alone were present, each particular element of a composite sound asserting its own individuality. Now, this is in strictness true only when the amplitudes of the oscillating particles are infinitely small. Guided by pure reasoning, the mathematician arrives at this result. The law is also practically true when the disturbances are *extremely* small; but it is *not* true after they have passed a certain limit. Vibrations which produce a large amount of disturbance give birth to secondary waves, which appeal to the ear as resultant tones. This has been proved by Helmholtz, and having proved this, he inferred further that there are also resultant tones formed by the *sum* of the primaries, as well as by their difference. He thus discovered the *summation tones* before he had heard them; and bringing his result to the test of experiment, he found that these tones had a real physical existence. They are not at all to be explained by Young's theory.

Another consequence of this departure from the law of superposition is, that a single sounding body, which disturbs the air beyond the limits of the law of superposition, also produces secondary waves, which correspond to the harmonic tones of the vibrating body. For example, the rate of vibration of the first overtone of a tuning-fork, as stated in the fourth Lecture, is $6\frac{1}{4}$ times the rate of the fundamental tone. But Helmholtz shows that a tuning-fork, not excited by a bow, but vigorously struck against a pad, emits the *octave* of its fundamental note, this octave being due to the secondary waves set up when the limits of the law of superposition have been exceeded.

These considerations make it probably evident to you that a coalescence of musical sounds is a far more complicated dynamical condition than you have hitherto sup-

posed it to be. In the music of an orchestra, not only have we the fundamental tones of every pipe and of every string, but we have the overtones of each, sometimes audible as far as the sixteenth in the series. We have also resultant tones; both difference tones and summation tones; all trembling through the same air, all knocking at the self-same tympanic membrane. We have fundamental tone interfering with fundamental tone; overtone with overtone; resultant tone with resultant tone. And besides this, we have the members of each class interfering with the members of every other class. The imagination retires baffled from any attempt to realise the physical condition of the atmosphere through which these sounds are passing. And, as we shall immediately learn, the aim of music, through the centuries during which it has ministered to the pleasure of man, has been to arrange matters empirically, so that the ear shall not suffer from the discordance produced by this multitudinous interference. The musicians engaged in this work knew nothing of the physical facts and principles involved in their efforts; they knew no more about it than the inventors of gunpowder knew about the law of atomic proportions. They tried and tried till they obtained a satisfactory result; and now, when the scientific mind is brought to bear upon the subject, order is seen rising through the confusion, and the results of pure empiricism are found to be in harmony with natural law.

SUMMARY OF LECTURE VIII.

When several systems of waves proceeding from distinct centres of disturbance pass through water or air, the motion of every particle is the algebraic sum of the several motions impressed upon it.

In the case of water, when the crests of one system of waves coincide with the crests of another system, higher waves will be the result of the coalescence of the two systems. But when the crests of one system coincide with the sinuses, or furrows, of the other system, the two systems, in whole or in part, destroy each other.

This coalescence and destruction of two systems of waves is called *interference*.

Similar remarks apply to sonorous waves. If in two systems of sonorous waves condensation coincides with condensation, and rarefaction with rarefaction, the sound produced by such coincidence is louder than that produced by either system taken singly. But if the condensations of the one system coincide with the rarefactions of the other, a destruction, total or partial, of both systems is the consequence.

Thus, when two organ-pipes of the same pitch are placed near each other on the same wind-chest and thrown into vibration, they so influence each other, that as the air enters the embouchure of the one it quits that of the other. At the moment, therefore, the one pipe produces a condensation, the other produces a rarefaction. The sounds of two such pipes mutually destroy each other.

When two musical sounds of nearly the same pitch

are sounded together, the flow of the sound is disturbed by *beats*.

These beats are due to the alternate coincidence and interference of the two systems of sonorous waves. If the two sounds be of the same intensity, their coincidence produces a sound of four times the intensity of either; while their opposition produces absolute silence.

The effect, then, of two such sounds, in combination, is a series of shocks, which we have called 'beats,' separated from each other by a series of 'pauses.'

The rate at which the beats succeed each other is equal to the difference between the two rates of vibration.

When a bell or disc sounds, the vibrations on opposite sides of the same nodal line partially neutralise each other; when a tuning-fork sounds, the vibrations of its two prongs in part neutralise each other. By cutting off a portion of the vibrations in these cases the sound may be intensified.

When a luminous beam, reflected on to a screen from two tuning-forks producing beats, is acted upon by the vibrations of both, the intermittence of the sound is announced by the alternate lengthening and shortening of the band of light upon the screen.

The soundless zones observed by General Duane on the coasts of the United States are shown to be due to the mutual extinction, by interference, of the direct sound-waves, and the waves reflected from the surface of the sea.

The law of the superposition of vibrations above enunciated is strictly true only when the amplitudes are exceedingly small. When the disturbance of the air by a sounding body is so violent that the law no longer holds good, secondary waves are formed, which correspond to the harmonic tones of the sounding body.

When two tones are rendered so intense as to exceed

the limits of the law of superposition, their secondary waves combine to produce *resultant tones.*

Resultant tones are of two kinds; the one class corresponding to rates of vibration equal to the difference of the rates of the two primaries; the other class corresponding to rates of vibration equal to the sum of the two primaries. The former are called *difference tones*, the latter *summation tones*

LECTURE IX.

COMBINATION OF MUSICAL SOUNDS—THE SMALLER THE TWO NUMBERS WHICH EXPRESS THE RATIO OF THEIR RATES OF VIBRATION, THE MORE PERFECT IS THE HARMONY OF TWO SOUNDS—NOTIONS OF THE PYTHAGOREANS REGARDING MUSICAL CONSONANCE—EULER'S THEORY OF CONSONANCE—THEORY OF HELMHOLTZ—DISSONANCE DUE TO BEATS—INTERFERENCE OF PRIMARY TONES AND OF OVERTONES—MECHANISM OF HEARING—SCHULTZE'S BRISTLES—THE OTOLITHES—CORTI'S FIBRES—GRAPHIC REPRESENTATION OF CONSONANCE AND DISSONANCE—MUSICAL CHORDS—THE DIATONIC SCALE—OPTICAL ILLUSTRATION OF MUSICAL INTERVALS—LISSAJOUS' FIGURES—SYMPATHETIC VIBRATIONS—VARIOUS MODES OF ILLUSTRATING THE COMPOSITION OF VIBRATIONS.

§ 1. *Musical Consonance.*

THE subject of this day's Lecture has two sides, a physical, and a psychical, the relationship of which we have now to determine. We have to-day to examine musical sounds in definite combination with each other, and to unfold the reason why some combinations are pleasant and others unpleasant to the ear.

Pythagoras made the first step towards the physical explanation of the musical intervals. This great philosopher stretched a string, and then divided it into three equal parts. At one of its points of division he fixed it firmly, thus converting it into two, one of which was twice the length of the other. He sounded the two sections of the string simultaneously, and found the note emitted by the short section to be the higher octave of that emitted by the long one. He then divided his string into two parts, bearing to each other the proportion of

2 : 3, and found that the notes were separated by an interval of a fifth. Thus, dividing his string at different points, Pythagoras found the so-called consonant intervals in music to correspond with certain lengths of his string; and he made the extremely important discovery, that *the simpler the ratio of the two parts into which the string was divided, the more perfect was the harmony of the two sounds.* Pythagoras went no further than this, and it remained for the investigators of a subsequent age to show that the strings act in this way in virtue of the relation of their lengths to the number of their vibrations. Why simplicity should give pleasure remained long an enigma, the only pretence of a solution being that of Euler, which, briefly expressed, is, that the human mind takes a constitutional delight in simple calculations.

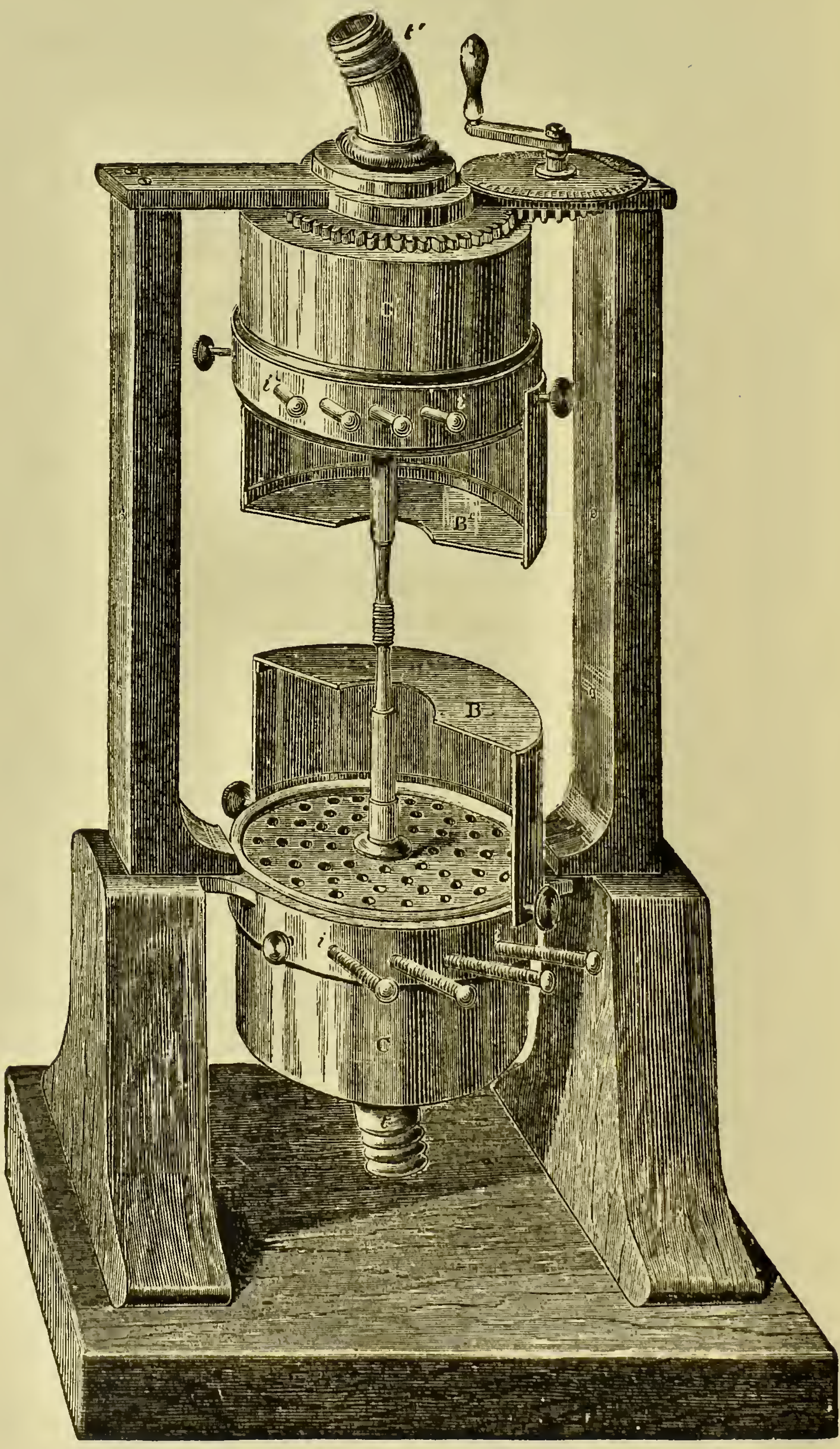
The double syren (fig. 178) enables us to obtain a great variety of combinations of musical sounds. And this instrument possesses over all others the advantage that, by simply counting the number of orifices corresponding respectively to any two notes, we obtain immediately the ratio of their rates of vibration. Before proceeding to these combinations I will enter a little more fully into the action of the double syren than has been hitherto deemed necessary or desirable.

The instrument, as already stated, consists of two of Dove's syrens, C′ and C, connected by a common axis, the upper one being turned upside down. Each syren is provided with four series of apertures, numbering as follows:—

	Upper syren. Number of apertures.	Lower syren. Number of apertures.
1st Series . . .	16	18
2nd Series . . .	15	12
3rd Series . . .	12	10
4th Series . . .	9	8

The number 12, it will be observed, is common to both

FIG. 178.



syrens. I open the two series of 12 orifices each, and urge air through the instrument; both sounds flow together in perfect unison; the unison being maintained, however the pitch may be exalted. We have however, already learned (Lecture II.) that by turning the handle of the upper syren, the orifices in its wind-chest C' are caused either to meet those of its rotating disc, or to retreat from them, the pitch of the upper syren being thereby raised or lowered. This change of pitch instantly announces itself by beats. The more rapidly the handle is turned, the more is the tone of the upper syren raised above or depressed below that of the lower one, and, as a consequence, the more rapid are the beats.

Now, the rotation of the handle is so related to the rotation of the wind-chest C' that when the handle turns through half a right angle the wind-chest turns through $\frac{1}{6}$th of a right angle, or through the $\frac{1}{24}$th of its whole circumference. But in the case now before us, where the circle is perforated by 12 orifices, the rotation through $\frac{1}{24}$th of its circumference causes the apertures of the upper wind-chest to be closed at the precise moments when those of the lower one are opened, and *vice versâ*. It is plain, therefore, that the intervals between the puffs of the lower syren, which correspond to the rarefactions of its sonorous waves, are here filled by the puffs, or condensations, of the upper syren. In fact, the condensations of the one coincide with the rarefactions of the other, and the absolute extinction of the sounds of both syrens is the consequence.

I may seem to you to have exceeded the truth here; for when the handle is placed in the position which corresponds to absolute extinction, you still hear a distinct sound. And, when the handle is turned continuously, though alternate swellings and sinkings of the tone occur, the sinkings by no means amount to absolute silence.

The reason is this. The sound of the syren is a highly composite one. By the suddenness and violence of its shocks, not only does it produce waves corresponding to the number of its orifices, but the aerial disturbance breaks up into secondary waves, which associate themselves with the primary waves of the instrument, exactly as the harmonics of a string, or of an open organ-pipe, mix with their fundamental tone. When the syren sounds, therefore, it emits, besides the fundamental tone, its octave, its twelfth, its double octave, and so on. That is to say, it breaks the air up into vibrations which have twice, three times, four times, &c., the rapidity of the fundamental one. Now, by turning the upper syren through $\frac{1}{24}$th of its circumference we extinguish utterly the fundamental tone. But we do not extinguish its octave.[1] Hence, when the handle is in the position which corresponds to the extinction of the fundamental tone, instead of silence, we have the full first harmonic of the instrument.

Helmholtz has surrounded both his upper and his lower syren with circular brass boxes, B, B′, each composed of two halves, which can be readily separated (one half of each box is removed in the figure). These boxes exalt by their resonance the fundamental tone of the instrument, and enable us to follow its variations much more easily than if it were not thus reinforced. It requires a certain rapidity of rotation to reach the maximum resonance of the brass boxes ; but when this speed is attained, the fundamental tone swells out with greatly augmented force, and, if the handle be then turned, the beats succeed each other with extraordinary power.

Still, as already stated, the pauses between the beats of the fundamental tone are not intervals of absolute

[1] Nor, indeed, any of those tones whose rates of vibration are *even* multiples of the rate of the fundamental.

silence, but are filled by the higher octave; and this renders caution necessary when the instrument is employed to determine rates of vibration. It is not without reason that I say so. Wishing to determine the rate of vibration of a small singing flame, I once placed a syren at some distance from it, sounded the instrument, and after a little time observed the flame dancing in synchronism with audible beats. I took it for granted that unison was nearly attained, and, under this assumption, determined the rate of vibration. The number obtained was surprisingly low—indeed, not more than half what it ought to be. What was the reason? Simply this; I was dealing, not with the fundamental tone of the syren, but with its higher octave. This octave and the flame produced beats by their coalescence; and hence the counter of the instrument, which recorded the rate, not of the octave, but of the fundamental, gave a number which was only half the true one. The fundamental tone was afterwards raised to unison with the flame. On approaching unison beats were again heard, and the jumping of the flame proceeded with an energy greater than that observed in the case of the octave. The counter of the instrument then recorded the accurate rate of the flame's vibration.

The tones first heard in the case of the syren are always overtones. These attain sonorous continuity sooner than the fundamental, flowing as smooth musical sounds while the fundamental tone is still in a state of intermittence. The syren is, however, so delicately constructed that a rate of rotation which raises the fundamental tone above its fellows, is almost immediately attained. And if we seek, by making the blast feeble, to keep the speed of rotation low, it is at the expense of intensity. Hence the desirability, if we wish to examine the overtones, of devising some means by which a strong blast and slow rotation shall be possible.

Helmholtz caused a spring to press as a light brake against the disc of the syren. Thus raising by slow degrees the speed of rotation, he was able deliberately to notice the predominance of the overtones at the commencement, and the final triumph of the fundamental tone. He did not trust to the direct observation of pitch, but determined the tone by the number of beats corresponding to one revolution of the handle of the upper syren. Supposing 12 orifices to be opened above and 12 below, the motion of the handle through 45° produces interference, and extinguishes the fundamental tone. The coincidences of that tone occur at the end of every rotation of 90°. Hence, for the fundamental tone, there must be *four* beats for every complete rotation of the handle. Now, Helmholtz, when he made the arrangement just described, found that the first beats numbered, not 4, but 12, for every revolution. They were, in fact, the beats, not of the fundamental tone, not even of the first overtone, but of the second overtone, whose rate of vibration is three times that of the fundamental. These beats continued as long as the number of air-shocks did not exceed 30 or 40 per second. When the shocks were between 40 and 80 per second, the beats fell from 12 to 8 for every revolution of the handle. Within this interval the first overtone, or the octave of the fundamental tone, was the most powerful, and made the beats its own. Not until the impulses exceeded 80 per second did the beats sink to 4 per revolution. In other words, not until the speed of rotation had passed this limit, was the fundamental tone able to assert its superiority over its companions.

This premised, we will combine the tones in definite order, while the cultivated ears here present shall judge of their musical relationship. The flow of perfect unison, when the two series of 12 orifices each are opened, has been already heard. I now open a series of 8 holes

in the upper, and of 16 in the lower syren. The interval you judge at once to be an octave. If a series of 9 holes in the upper and of 18 holes in the lower syren be opened, the interval is still an octave. This proves that the interval is not disturbed by altering the absolute rates of vibration, so long as the *ratio* of the two rates remains the same. The same truth is more strikingly illustrated by commencing with a low speed of rotation, and urging the syren to its highest pitch; as long as the orifices are in the ratio of 1 : 2, we retain the constant interval of an octave. Opening a series of 10 holes in the upper, and of 15 in the lower syren, the ratio is as 2 : 3, and every musician present knows that this is the interval of a fifth. Opening 12 holes in the upper, and 18 in the lower syren, does not change the interval. Opening two series of 9 and 12, or of 12 and 16, we obtain an interval of a fourth; the ratio in both these cases being as 3 : 4. In like manner two series of 8 and 10, or of 12 and 15, give us the interval of a major third; the ratio in this case being as 4 : 5. Finally, two series of 10 and 12, or of 15 and 18, yield the interval of a minor third, which corresponds to the ratio 5 : 6.

These experiments amply illustrate two things:—firstly, that a musical interval is determined, not by the absolute number of vibrations of the two combining notes. but by the ratio of their vibrations. Secondly, and this is of the utmost significance, that the smaller the two numbers which express the ratio of the two rates of vibration, the more perfect is the consonance of the two sounds. The most perfect consonance is the unison 1 : 1; next comes the octave 1 : 2; after that the fifth 2 : 3; then the fourth 3 : 4; then the major third 4 : 5; and finally the minor third 5 : 6. We can also open two series numbering, respectively, 8 and 9 orifices: this interval corresponds to *a tone* in music. It is a dissonant

combination. Two series which number respectively 15 and 16 orifices make the interval of a *semi-tone*: it is a very sharp and grating dissonance.

§ 2. *The Theory of Musical Consonance. Pythagoras and Euler.*

Whence, then, does this arise? Why should the smaller ratio express the more perfect consonance? The ancients attempted to solve this question. The Pythagoreans found intellectual repose in the answer 'all is number and harmony.' The numerical relations of the seven notes of the musical scale were also thought by them to express the distances of the planets from their central fire; hence the choral dance of the worlds, the 'music of the spheres,' which, according to his followers, Pythagoras alone was privileged to hear. And might we not in passing contrast this exalted superstition with the grovelling delusions which have taken hold of the fantasy of our day? Were the character which superstition assumes in different ages, an indication of man's advance or retrogression, assuredly the nineteenth century would have no reason to plume itself, in comparison with the sixth, B.C. A more earnest attempt to account for the more perfect consonance of the smaller ratios was made by the celebrated mathematician, Euler, and his explanation, if such it could be called, long silenced, if it did not satisfy, inquirers. Euler analyses the cause of pleasure. We take delight in *order*; it is pleasant to us to observe means 'co-operant to an end.' But, then, the effort to discern order must not be so great as to weary us. If the relations to be disentangled are too complicated, though we may see the order, we cannot enjoy it. The simpler the terms in which the order expresses itself, the greater is our delight. Hence, the superiority of the simpler

ratios in music over the more complex ones. Consonance, then, according to Euler, was the satisfaction derived from the perception of order without weariness of mind.

But in this theory it was overlooked that Pythagoras himself, who first experimented on these musical intervals, knew nothing about rates of vibration. It was forgotten, that the vast majority of those who take delight in music, and who have the sharpest ears for the detection of a dissonance, are in the condition of Pythagoras, knowing nothing whatever about rates or ratios. And it may also be added that the scientific man, who is fully informed upon these points, has his pleasure in no way enhanced by his knowledge. Euler's explanation, therefore, does not satisfy the mind, and it was reserved for an eminent German investigator of our own day, after a profound analysis of the entire question, to assign the physical cause of consonance and dissonance—a cause which, when once clearly stated, is so simple and satisfactory as to excite surprise that it remained so long without a discoverer.

Various expressions employed in our previous Lectures have already, in part, forestalled Helmholtz's explanation of consonance and dissonance. Let me here repeat an experiment which will, almost of itself, force this explanation upon your attention. Before you are two jets of burning gas, which can be converted into singing flames by inclosing them within two tubes. The tubes are of the same length, and the flames are now singing in unison. By means of a telescopic slider I lengthen slightly one of the tubes; you hear beats, which succeed each other so slowly that they can readily be counted. I augment still further the length of the tube. The beats are now more rapid than before: they can barely be counted. It is perfectly manifest that the shocks of which you are now sensible, differ only in point of rapidity

from the slow beats which you heard a moment ago. There is no breach of continuity here. We begin slowly, we gradually increase the rapidity, until finally the succession of the beats is so rapid as to produce that particular grating effect which every musician that hears it would call *dissonance.* Let us now reverse the process, and pass from these quick beats to slow ones. The same continuity of the phenomenon is noticed. By degrees the beats separate from each other more and more, until finally they are slow enough to be counted. Thus these singing flames enable us to follow the beats with certainty, until they cease to be beats, and are converted into dissonance.

This experiment proves conclusively that dissonance *may* be produced by a rapid succession of beats; and I imagine this cause of dissonance would have been pointed out earlier, had not men's minds been thrown off the proper track by the theory of 'resultant tones' enunciated by Thomas Young. Young imagined that, when they were quick enough, the beats ran together to form a resultant tone. He imagined the linking together of the beats to be precisely analogous to the linking together of simple musical impulses; and he was strengthened in this notion by the fact already adverted to, that the first difference tone—that is to say, the loudest resultant tone—corresponded, as the beats do, to a rate of vibration equal to the difference of the rates of the two primaries. The fact, however, is, that the effect of beats upon the ear is altogether different from that of the successive impulses of an ordinary musical tone.

§ 3. *Sympathetic Vibrations.*

But to grasp, in all its fullness, the new theory of musical consonance, some preliminary studies will be necessary.

And here I would ask you to call to mind the experiments (in Lecture III.) by which the division of a string into its harmonic segments was illustrated. This was done by means of little paper riders, which were unhorsed, or not, according as they occupied a ventral segment or a node upon the string. Before you at present is the sonometer, employed in the experiments just referred to. Along it, instead of one, are stretched two strings, about three inches asunder. By means of a key these strings are brought into unison. And now I place a little paper rider upon the middle of one of them, and agitate the other. What occurs? The vibrations of the sounding string are communicated to the bridges on which it rests, and through the bridges to the other string. The individual impulses are very feeble, but, because the two strings are in unison, the impulses can so accumulate as finally to toss the rider off the untouched string.

Every experiment executed with the riders and a single string, may be repeated with these two unisonant strings. Damping, for instance, one of the strings, at a point one-fourth of its length from one of its ends, and placing the red and blue riders formerly employed, not on the nodes and ventral segments of the damped string, but at points upon the second string exactly opposite to those nodes and segments; when the bow is passed across the shorter segment of the damped string, the five red riders on the adjacent string are unhorsed, while the four blue ones remain tranquilly in their places. By relaxing one of the strings, it is thrown out of unison with the other, and then all efforts to unhorse the riders are unavailing. That accumulation of impulses, which unison alone renders possible, cannot here take place, and the consequence is that, however great the agitation of the one string may be, it fails to produce any sensible effect upon the other.

The influence of synchronism may be illustrated in a still more striking manner, by means of two tuning-forks which sound the same note. Two such forks mounted on their resonant supports are placed upon the table. I draw the bow vigorously across one of them, permitting the other fork to remain untouched. On stopping the agitated fork, the sound is enfeebled, but by no means quenched. Through the air and through the wood the vibrations have been conveyed from fork to fork, and the untouched fork is the one you now hear. When, by means of a morsel of wax, a small coin is attached to one of the forks, its power of influencing the other ceases; the change in the rate of vibration, if not very small, so destroys the sympathy between the two forks, as to render a response impossible. On removing the coin the untouched fork responds as before.

This communication of vibrations through wood and air may be obtained when the forks, mounted on their cases, stand several feet apart. But the vibrations may also be communicated through the air alone. Holding the resonant case of a vigorously vibrating fork in my hand, I bring one of its prongs near an unvibrating one, placing the prongs back to back, but allowing a space of air to exist between them. Light as is the vehicle, the accumulation of impulses, secured by the perfect unison of the two forks, enables the one to set the other in vibration. Extinguishing the sound of the agitated fork, that which a moment ago was silent continues sounding, having taken up the vibrations of its neighbour. Removing one of the forks from its resonant case, and striking it against a pad, it is thrown into strong vibration. Held free in the air, its sound is inaudible. But, on bringing it close to the silent mounted fork, out of the silence rises a full mellow sound, which is due, not to the fork originally agitated, but to its sympathetic neighbour.

Various other examples of the influence of synchronism, already brought forward, will occur to you here; and cases of the kind might be indefinitely multiplied. If two clocks, for example, with pendulums of the same period of vibration, be placed against the same wall, and if one of the clocks be set going and the other not, the ticks of the moving clock, transmitted through the wall, will act upon its neighbour. The quiescent pendulum moved by a single tick, swings through an extremely minute arc; but it returns to the limit of its swing just in time to receive another impulse. By the continuance of this process, the impulses so add themselves together as finally to set the clock a-going. It is by this timing of impulses that a properly pitched voice can cause a glass to ring, and that the sound of an organ can break a particular window-pane.

§ 4. *Sympathetic Vibration in relation to the Human Ear.*

If I dwell so fully upon this subject, it is for the purpose of rendering intelligible the manner in which sonorous motion is communicated to the auditory nerve. In the organ of hearing, in man, we have first of all the external orifice of the ear, closed at the bottom by the circular tympanic membrane. Behind that membrane is the drum of the ear, this cavity being separated from the space between it and the brain by a bony partition, in which there are two orifices, the one round and the other oval. These orifices are also closed by fine membranes. Across the drum stretches a series of four little bones. The first, called the *hammer*, is attached to the tympanic membrane; the second, called the *anvil*, is connected by a joint with the hammer; a third little round bone connects the anvil with the *stirrup bone*, the base of which

is planted against the membrane of the oval orifice just referred to. This oval membrane is almost covered by the stirrup bone, a narrow rim only of the membrane surrounding the bone being left uncovered. Behind the bony partition, and between it and the brain, we have the extraordinary organ called the *labyrinth*, filled with water, over the lining membrane of which are distributed the terminal fibres of the auditory nerve. When the tympanic membrane receives a shock, it is transmitted through the series of bones above referred to, being concentrated on the membrane against which the base of the stirrup bone is fixed. The membrane transfers the shock to the water of the labyrinth, which, in its turn, transfers it to the nerves.

The transmission, however, is not direct. At a certain place within the labyrinth exceedingly fine elastic bristles, terminating in sharp points, grow up between the terminal nerve fibres. These bristles, discovered by Max Schultze, are eminently calculated to sympathise with such vibrations of the water as correspond to their proper periods. Thrown thus into vibration, the bristles stir the nerve fibres which lie between the roots of the bristles. At another place in the labyrinth we have little crystalline particles called *otolithes*—the Hörsteine of the Germans—embedded among the nervous filaments, which, when they vibrate, exert an intermittent pressure upon the adjacent nerve fibres. The otolithes probably serve a different purpose from that of the bristles of Schultze. They are fitted, by their weight, to accept and prolong the vibrations of evanescent sounds, which might otherwise escape attention, while the bristles of Schultze, because of their extreme lightness, would instantly yield up an evanescent motion. The bristles, on the other hand, are eminently fitted for the transmission of continuous vibrations.

Finally, there is in the labyrinth an organ, discovered by the Marchese Corti, which is to all appearance a musical instrument, with its chords so stretched as to accept vibrations of different periods, and transmit them to the nerve filaments which traverse the organ. Within the ears of men, and without their knowledge or contrivance, this lute of 3,000 strings has existed for ages, accepting the music of the outer world and rendering it fit for reception by the brain.[1] Each musical tremor which falls upon this organ selects from the stretched fibres the one appropriate to its own pitch, and throws it into unisonant vibration. And thus, no matter how complicated the motion of the external air may be, these microscopic strings can analyse it and reveal the constituents of which it is composed. Surely, inability to feel the stupendous wonder of what is here revealed would imply incompleteness of mind.

§ 5. *Consonant Intervals in relation to the Human Ear.*

This view of the use of Corti's fibres is theoretical; but it comes to us commended by every appearance of truth. It will enable us to tie together many things, whose relations it would be otherwise difficult to discern. When a musical note is sounded its corresponding Corti's fibre resounds, being moved, as a string is moved by a second unisonant string. And when two sounds coalesce to produce beats, the intermittent motion is transferred to the proper fibre within the ear. But here it is to be noted, that for the same fibre to be affected simultaneously by two different sounds, it must not be far removed in pitch from either of them. Call to mind our repetition of Melde's experiments (in Lecture III.). You then had frequent occasion to notice, that even before perfect syn-

[1] According to Kölliker, this is the number of fibres in Corti's organ.

chronism had been established between the string and the tuning-fork to which it was attached, the string began to respond to the fork. But you also noticed how rapidly the vibrating amplitude of the string increased, as it came close to perfect synchronism with the vibrating fork. On approaching unison the string would open out, say to an amplitude of an inch; and then a slight tightening or slackening, as the case might be, would bring it up to unison, and cause it to open out suddenly to an amplitude of six inches.

So also in reference to the experiment made a moment ago with the sonometer; you noticed that the unhorsing of the paper riders was preceded by a fluttering of the bits of paper; showing that the sympathetic response of the second string had begun, though feebly, prior to perfect synchronism. Instead of two strings, conceive three strings, all nearly of the same pitch, to be stretched upon the sonometer; and suppose the vibrating period of the middle string to lie midway between the periods of its two neighbours, being a little higher than the one and a little lower than the other. Each of the side strings, sounded singly, would cause the middle string to respond. Sounding the two side strings together they would produce beats; the corresponding intermittence would be propagated to the central string, which would beat in synchronism with the beats of its neighbours. In this way we make plain to our minds how a Corti's fibre may, to some extent, take up the vibrations of a note, nearly, but not exactly, in unison with its own; and that when two notes close to the pitch of the fibre act upon it together, their beats are responded to by an intermittent motion on the part of the fibre. This power of sympathetic vibration would fall rapidly on both sides of the perfect unison, so that on increasing the interval between the two notes, a time would soon arrive when the same

fibre would refuse to be acted on simultaneously by both. Here the condition of the organ, necessary for the perception of audible beats, would cease.

In the middle region of the pianoforte, with the interval of a semitone, the beats are sharp and distinct, falling indeed upon the ear as a grating dissonance. Extending the interval to a whole tone, the beats become more rapid, but less distinct. With the interval of a minor third between the two notes, the beats in the middle region of the scale cease to be sensible. But this smoothening of the sound is not wholly due to the augmented rapidity of the beats. It is due in part to the fact, for which the foregoing considerations have prepared us, that the two notes here sounded are too far removed from that of the intermediate Corti's fibre to affect it powerfully. By ascending to the higher regions of the scale we can produce, with a narrower interval than the minor third, the same, or even a greater number of beats, which are sharply distinguishable because of the closeness of their component notes. In the very highest regions of the scale, however, the beats, when they become very rapid, cease to appeal as roughness to the ear.

Hence both the rapidity of the beats, and the width of the interval, enter into the question of consonance. Helmholtz judges that in the middle and higher regions of the musical scale, when the beats reach 33 per second, the dissonance reaches its maximum. Both slower and quicker beats have a less grating or dissonant effect. When the beats are very slow, they may be of advantage to the music; and when they reach 132 per second, their roughness is no longer discernible.

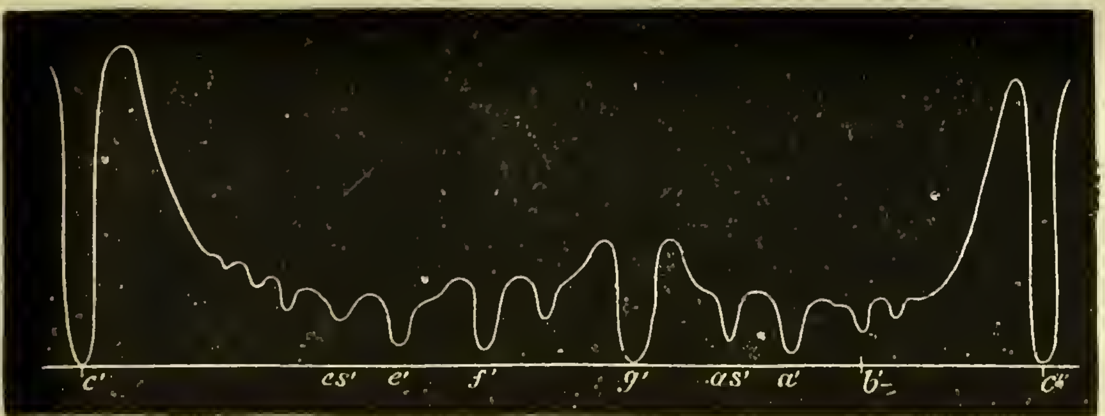
Thanks to Helmholtz, whose views I have here sought to express in the briefest possible language, we are now in a condition to grapple with the question of musical intervals, and to give the reason why some are consonant and

some dissonant to the ear. Circumstanced as we are upon earth, all our feelings and emotions, from the lowest sensation to the highest æsthetic consciousness, have a mechanical cause: though it may be for ever denied to us to take the step from cause to effect; or to understand why the agitations of nervous matter can awaken the delights which music imparts. Take, then, the case of a violin. The fundamental tone of every string of this instrument is demonstrably accompanied by a crowd of overtones; so that when two violins are sounded, we have not only to take into account the consonance, or dissonance, of the fundamental tones, but also those of the higher tones of both. Supposing two strings sounded whose fundamental tones, and all of whose partial tones, coincide, we have then absolute unison; and this we actually have when the ratio of vibration is 1 : 1. So also when the ratio of vibration is accurately 1 : 2, each overtone of the fundamental finds itself in absolute coincidence with either the fundamental tone or some higher tone of the octave. There is no room for beats or dissonance. When we examine the interval of a fifth, with a ratio of 2 : 3, we find the coincidence of the partial tones of the two strings so perfect as almost, though not wholly, to exclude every trace of dissonance. Passing on to the other intervals, we find the coincidence of the partial tones less perfect, as the numbers expressing the ratio of the vibrations become larger. Thus the dissonance of intervals whose rates of vibration can only be expressed by large numbers, is not to be ascribed to any mystic quality of the numbers themselves; but to the fact, that the fundamental tones which require such numbers are inexorably accompanied by partial tones whose coalescence produces beats, these producing the grating effect known as dissonance.

§ 6. *Graphic Representation of Consonance and Dissonance.*

Helmholtz has attempted to represent this result graphically, and from his work I copy, with some modification, the next two diagrams. He assumes, as already stated, the maximum dissonance to correspond to 33 beats per second; and he seeks to express different degrees of dissonance by lines of different lengths. The horizontal line *c′ c″*, fig. 179, represents a range of the musical scale in which *c″* is our middle C, with 528 vibrations, and *c′*

FIG. 179.

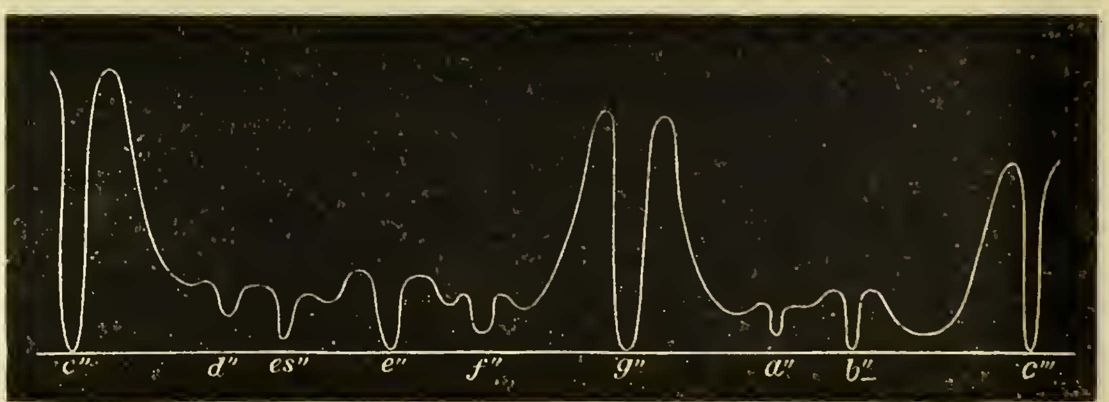


the lower octave of *c″*. The distance from any point of this line to the curve above it represents the dissonance corresponding to that point. The pitch here is supposed to ascend continuously, and not by jumps. Supposing, for example, two performers on the violin to start with the same note *c′*, and that, while one of them continues to sound that note, the other gradually and continuously shortens his string, thus gradually raising its pitch up to the octave *c″*. The effect upon the ear would be represented by the irregular curved line in fig. 179. Soon after the unison, which is represented by contact at *c′*, is departed from, the curve suddenly rises, showing the dissonance here to be the sharpest of all. At *e′*, the curve

approaches the straight line c' c'', and this point corresponds to the major third. At f' the approach is still nearer, and this point corresponds to the fourth. At g' the curve almost touches the straight line, indicating that at this point, which corresponds to the fifth, the dissonance almost vanishes. At a' we have the major sixth ; while at c'', where the one note is an octave above the other, the dissonance entirely vanishes. The *e s'* and the *a s'*, of this diagram, are the German names of a flat third and a flat sixth.

Maintaining the same fundamental note c', and passing through the octave above c'', the various degrees of conso-

Fig. 180.



nance and dissonance are those shown in fig. 180. That is to say, beginning with the octave c'—c'', and gradually elevating the pitch of one of the strings, till it reaches c''', the octave of c'', the curved line represents the effect upon the ear. We see, from both these curves, that dissonance is the general rule, and that only at certain definite points does the dissonance vanish, or become so decidedly enfeebled as not to destroy the harmony. These points correspond to the places where the numbers expressing the ratio of the two rates of vibration are small whole numbers. It must be remembered that these curves are constructed on the supposition that the beats are the cause of the dissonance ; and the agreement between calculation and

experience sufficiently demonstrates the truth of the assumption.[1]

You have thus accompanied me to the verge of the Physical portion of the science of Acoustics, and through the æsthetic portion I have not the knowledge of music necessary to lead you. I will only add, that in comparing three or more sounds together—that is to say, in choosing them for *chords*—we are guided by the principles just mentioned. We choose sounds which are in harmony with the fundamental sound and with each other. In choosing a series of sounds for combination two by two, the simplicity alone of the ratios would lead us to fix on those expressed by the numbers $1, \frac{5}{4}, \frac{4}{3}, \frac{3}{2}, \frac{5}{3}, 2$; these being the simplest ratios that we can have within an octave. But when the notes represented by these ratios are sounded in succession, it is found that the intervals between 1 and $\frac{5}{4}$, and between $\frac{5}{3}$ and 2, are wider than the others, and require the interpolation of a note in each case. The notes chosen are such as form chords, not with the fundamental tone, but with the note $\frac{3}{2}$ regarded as a fundamental tone. The ratios of these two notes with the fundamental are $\frac{9}{8}$ and $\frac{15}{8}$. Interpolating these, we have the eight notes of the natural or diatonic scale, expressed by the following names and ratios :—

Names.	C.	D.	E.	F.	G.	A.	B.	C.
Intervals	1st.	2nd.	3rd.	4th.	5th.	6th.	7th.	8th.
Rates of vibration.	1,	$\frac{9}{8}$,	$\frac{5}{4}$,	$\frac{4}{3}$,	$\frac{3}{2}$,	$\frac{5}{3}$,	$\frac{15}{8}$,	2.

Multiplying these ratios by 24, to avoid fractions, we obtain the following series of whole numbers, which express the relative rates of vibration of the notes of the diatonic scale :—

24, 27, 30, 32, 36, 40, 45, 48.

[1] Considering the above curve to represent a mountain chain, Mr. Sedley Taylor calls the discords *peaks*, and the concords *passes*.

The meaning of the terms third, fourth, fifth, &c., which we have so often applied to the musical intervals, is now apparent; the term has reference to the position of the note in the scale.

§ 7. *Composition of Vibrations.*

In our second Lecture I referred to, and in part illustrated, a method devised by M. Lissajous for studying musical vibrations. By means of a beam of light reflected from a mirror attached to a tuning-fork, the fork was made to write the story of its own motion. In our last Lecture the same method was employed to illustrate optically the phenomenon of beats. I now propose to apply it to the study of the composition of the vibrations which constitute the principal intervals of the diatonic scale. We must, however, prepare ourselves for the thorough comprehension of this subject by a brief preliminary examination of the vibrations of a common pendulum.

Such a pendulum hangs before you. It consists of a wire carefully fastened to a plate of iron at the roof of the house, and bearing a copper ball weighing 10 lbs. I draw the pendulum aside and let it go; it oscillates to and fro almost in the same plane.

I say 'almost,' because it is practically impossible to suspend a pendulum without some little departure from perfect symmetry around its point of attachment. In consequence of this, the weight deviates sooner or later from a straight line, and describes an oval more or less elongated. Some years ago this circumstance presented a serious difficulty to those who wished to repeat M. Foucault's celebrated experiment, demonstrating the rotation of the earth.

Nevertheless, in the case now before us, the pendulum is so carefully suspended, that its deviation from a

straight line is not at first perceptible. Let us suppose the amplitude of its oscillation to be represented by the dotted line *a b*, fig. 181. The point *d*, midway between *a* and *b*, is the pendulum's point of rest. When drawn aside from this point to *b*, and let go, it will return to *d*, and in virtue of its momentum will pass on to *a*. There it comes momentarily to rest, and returns through *d* to *b*. And thus it will continue to oscillate until its motion is expended.

Fig. 181.

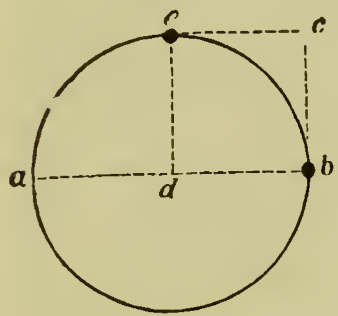
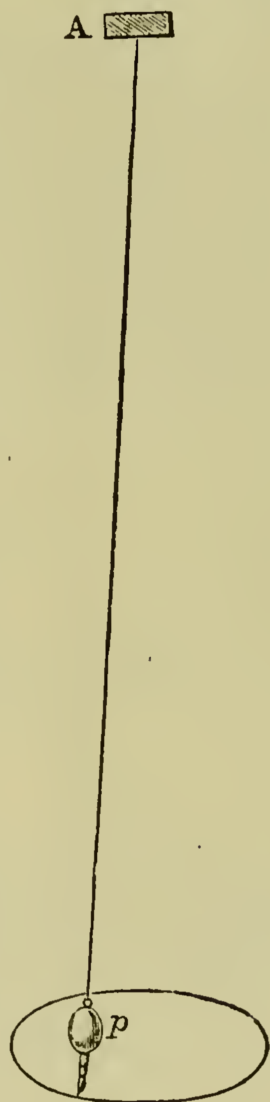


Fig. 182.



The pendulum having first reached the limit of its swing at *b*, let us suppose a push in a direction perpendicular to *a b* imparted to it; that is to say, in the direction *b c*. Supposing the time required by the pendulum to swing from *b* to *a* to be one second,[1] then the time required to swing from *b* to *d* will be half a second. Suppose, further, the force applied at *b* to be such as would carry the bob, if free to move in that direction alone, to *c* in half a second, and that the distance *b c* is equal to *b d*, the question then occurs where will the bob really find itself at the end of half a second? It is perfectly manifest that both forces are satisfied by the pendulum reaching the point *e*, exactly opposite the centre *d*, in half a second. To reach this point it can be shown

[1] This supposition is of course made for the sake of simplicity, the real period of oscillation of a pendulum 28 feet long being between two and three seconds.

that it must describe the circular arc *b e*, and it will pursue its way along the continuation of the same arc, to *a*, and then pass round to *b*. Thus, by the rectangular impulse the rectilinear oscillation is converted into a rotation, the pendulum describing a circle, as shown in fig. 182.

If the force applied at *b* be sufficient to urge the weight in half a second through a greater distance than *b c*, the pendulum will describe an ellipse, with the line *a b* for its smaller axis; if, on the contrary, the force applied at *b* urge the pendulum in half a second through a distance less than *b c*, the weight will describe an ellipse, with the line *a b* or its greater axis.

Fig. 183.

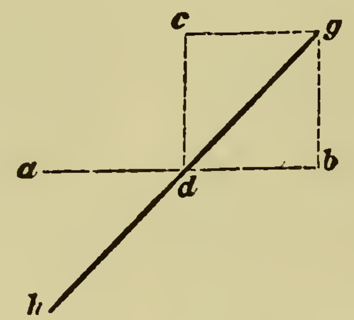
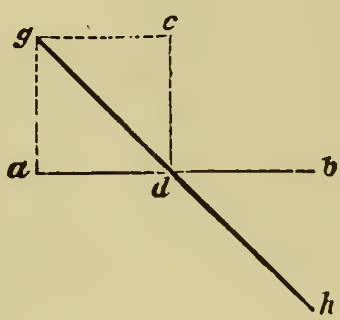


Fig. 184.



Let us now inquire what occurs when the rectangular impulse is applied at the moment the ball is passing through its position of rest at *d*.

Supposing the pendulum to be moving from *a* to *b*, fig. 183, and that at *d* a shock is imparted to it sufficient of itself to carry it in half a second to *c*; it is here manifest that the resultant motion will be along the straight line *d g* lying between *d b* and *d c*. The pendulum will return along this line to *d*, and pass on to *h*. In this case, therefore, the pendulum will describe a straight line, *g h*, oblique to its original direction of oscillation.

Supposing the direction of motion, at the moment the push is applied, to be from *b* to *a*, instead of from *a* to *b*, it is manifest that the resultant here will also be a straight

line oblique to the primitive direction of oscillation; but its obliquity will be that shown in fig. 184.

When the impulse is imparted to the pendulum neither at the centre nor at the limit of its swing, but at some point between both, we obtain neither a circle nor a straight line, but something between both. We have, in fact, a more or less elongated ellipse with its axis oblique to *a b*, the original direction of vibration. If, for example, the impulse be imparted at *d′*, fig. 185, while the pendulum is moving towards *b*, the position of the ellipse will be that shown in fig. 185; but if the push at *d′* be given when the motion is towards *a*, then the position of the ellipse will be that represented in fig. 186.

Fig. 185.

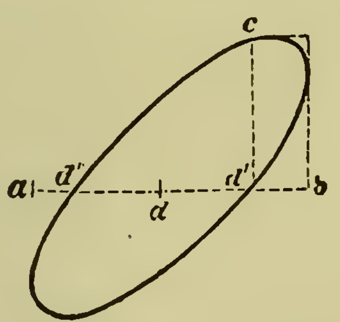
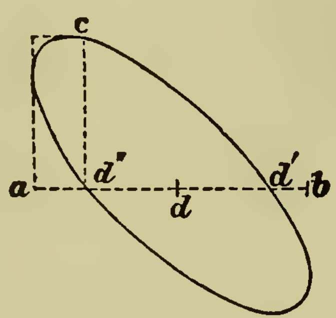


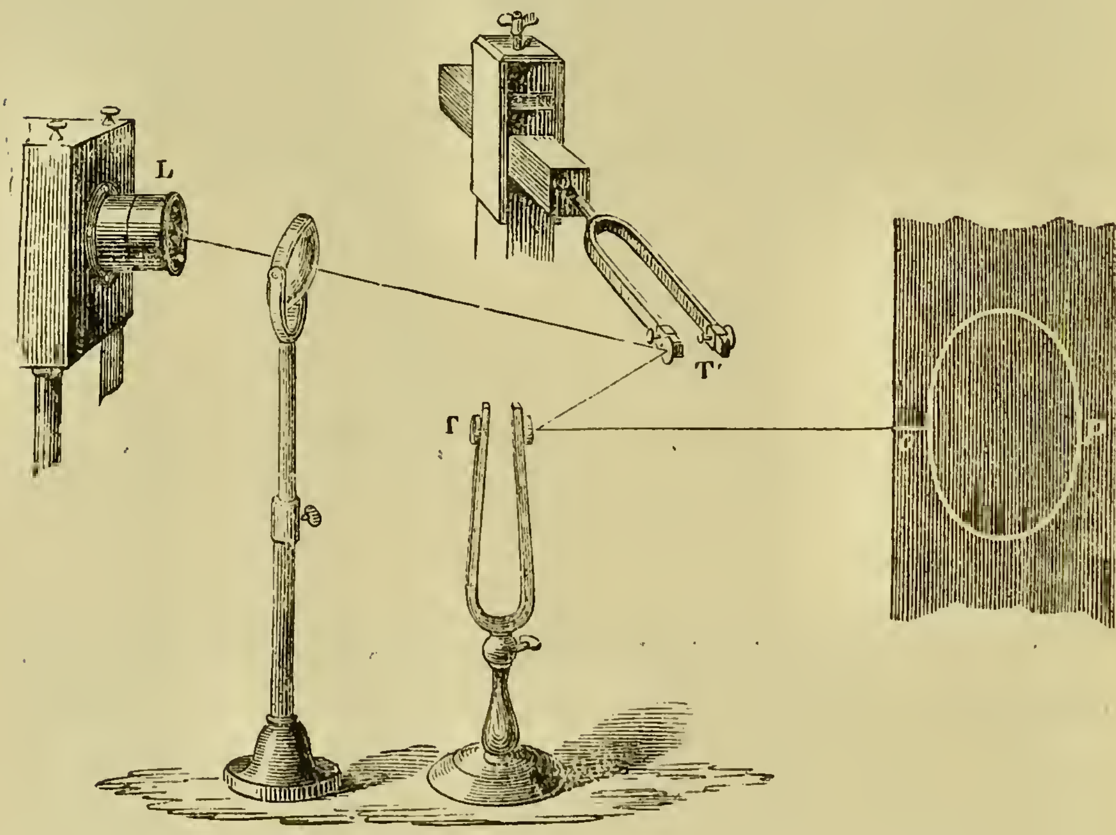
Fig. 186.



By the method of M. Lissajous we can combine the rectangular vibrations of two tuning-forks—a subject which I now wish to illustrate before you. In front of an electric lamp, L, fig. 187, is placed a large tuning-fork, T′, fixed in a stand horizontally, and provided with a mirror, on which a narrow beam of light, L T′, is permitted to fall. The beam is thrown back, by reflection. In the path of the reflected beam is placed a second upright tuning-fork, T, also furnished with a mirror. By the horizontal fork, when it vibrates, the beam is tilted laterally; by the vertical fork, vertically. At the present moment both forks are motionless, the beam of light being reflected from the mirror of the horizontal to that of the vertical fork, and from the latter to the screen, on

which it prints a brilliant disc. I now agitate the upright fork, leaving the other motionless. The disc is drawn out into a fine luminous band, 3 feet long. On sounding the second fork, the straight band is instantly transformed into a white ring *o p*, fig. 187, 36 inches in diameter. What have we done here? Exactly what we did in our first experiment with the pendulum. We

FIG. 187.



have caused a beam of light to vibrate simultaneously in two directions, and have accidentally hit upon the phase when one fork has just reached the limit of its swing, and come momentarily to rest, while the beam is receiving the maximum impulse from the other fork.

That the *circle* was obtained is, as stated, a mere accident; but it was a fortunate accident, as it enables us to see the exact similarity between the motion of the beam and that of the pendulum. I stop both forks, and agitating them afresh, obtain an ellipse with its axis

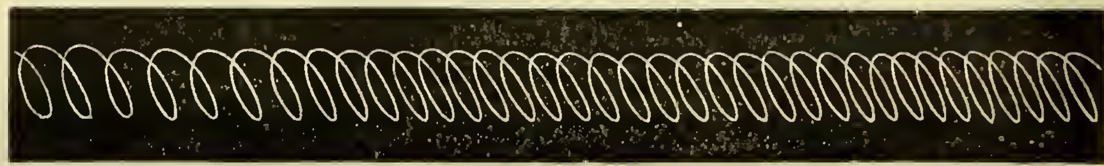
oblique. After a few trials we obtain the straight line, indicating that both the forks then pass simultaneously through their positions of equilibrium. In this way, by combining the vibrations of the two forks, we reproduce all the figures obtained with the pendulum.

When the vibrations of the two forks are, in all respects, absolutely alike, whatever the figure may be which is first traced upon the screen, it remains unchanged in form, diminishing only in size as the motion is expended. But the slightest difference in the rates of vibration destroys this fixity of the image. I endeavoured before the lecture to render the unison between these two forks as perfect as possible, and hence you have observed very little alteration in the shape of the figure. But by moving a small weight along the prong of either fork, or by attaching to either of them a bit of wax, the unison is impaired. The figure then obtained by the combination of both passes slowly from a straight line into an oblique ellipse, thence into a circle; after which it narrows again to an ellipse with an opposed obliquity; it then passes again into a straight line, the direction of which is at right angles to the first direction. Finally it passes, in the reverse order, through the same series of figures to the straight line with which we began. The interval between two successive identical figures is the time in which one of the forks succeeds in executing one complete vibration more than the other. Loading the fork still more heavily, we have more rapid changes; the straight line, ellipse, and circle being passed through in quick succession. At times the luminous curve exhibits a stereoscopic depth, which renders it difficult to believe that we are not looking at a solid ring of white-hot metal.

By causing the mirror of the fork, T, to rotate through a small arc, the steady circle first obtained is drawn out

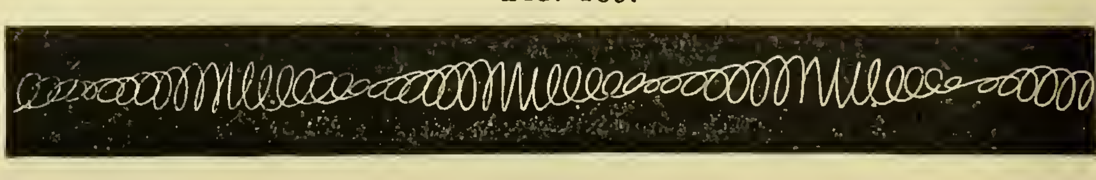
into a luminous scroll stretching right across the screen, fig. 188. The same experiment made with the changing

FIG. 188.



figure, obtained by throwing the forks out of unison, gives us a scroll of periodically varying amplitude, fig. 189.[1]

FIG. 189.

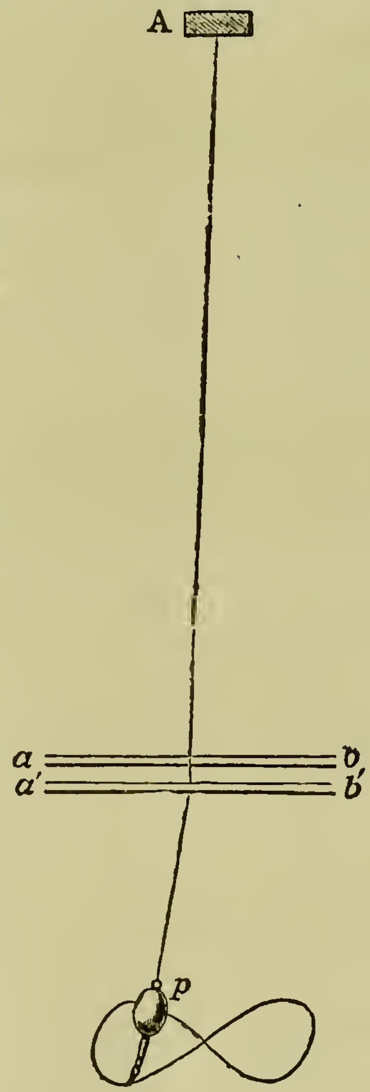


We have next to combine the vibrations of two forks one of which oscillates with twice the rapidity of the other; in other words, to determine the figure corresponding to the combination of a note and its octave. To prepare ourselves for the mechanics of the problem, we must resort once more to our pendulum; for it also can be caused to oscillate in one direction twice as rapidly as in another. By a complicated mechanical arrangement this might be done in a very perfect manner, but at present simplicity is preferable to completeness. The wire of our pendulum is therefore permitted to descend from its point of suspension, A, fig. 190, midway between two horizontal glass rods, *a b*, *a′ b′*, supported firmly at their ends, and about an inch asunder. The rods cross the wire at a height of 7 feet above the bob of the pendulum. The whole length of the pendulum being 28 feet, the glass rods intercept one-fourth of this length. On drawing the pendulum aside in the direction of the rods, *a b*, *a′ b′*, and letting it go, it oscillates freely between them.

[1] This figure corresponds to the interval 15 : 16. For it and some other figures, I am indebted to that excellent mechanician, M. König of Paris.

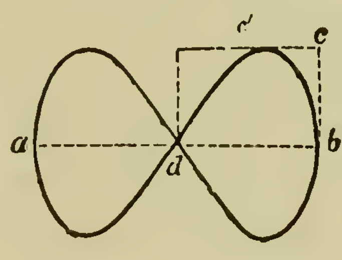
I bring it to rest and draw it aside in a direction perpendicular to the last; a length of 7 feet only can now oscillate, and by the laws of oscillation a pendulum 7 feet long vibrates with twice the rapidity of a pendulum 28 feet long.

Fig. 190.



I wish to show you the figure described by the combination of these two rates of vibration. Attached to the copper ball, p, is a camel's hair pencil, intended to rub lightly upon a glass plate placed on black paper, and over which is strewn white sand. Allowing the pendulum to oscillate as a whole, the sand is rubbed away along a straight line which represents the amplitude of the vibration. Let *a b*, fig. 191, represent this line, which, as before, we will assume to be described in one second. When the pendulum is at the limit, *b*, of its swing, let a rectangular impulse be imparted to it sufficient to carry it to *c* in one-fourth of a second. If this were the only impulse acting on the pendulum, the bob would reach *c* and return to *b* in half a second. But under the actual circumstances it is also urged towards *d*, which point, through the vibration of the whole pendulum, it ought also to reach in half a second. Both vibrations, therefore, require that the bob shall reach *d* at the same moment; and to do this it will have to describe the curve *b c′ d*. Again, in the time required by the long pen-

Fig. 191.

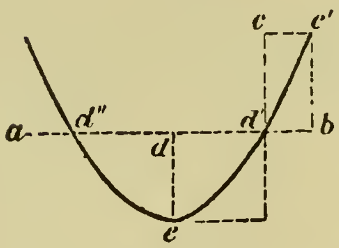


dulum to pass from *d* to *a*, the short pendulum will pass *to and fro* over the half of its excursion; both vibrations must therefore reach *a* at the same moment, and to accomplish this the pendulum describes the lower curve between *d* and *a*. It is manifest that these two curves will repeat themselves at the opposite sides of *a b*, the combination of both vibrations producing finally a figure of 8, which you now see fairly drawn upon the sand before you.

The same figure is obtained if the rectangular impulse be imparted when the pendulum is passing its position of rest, *d*.

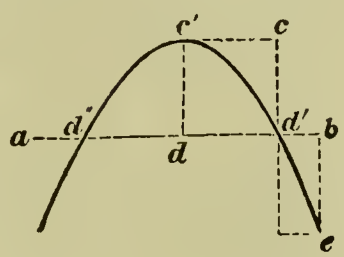
I have here supposed the time occupied by the pendulum in describing the line *a b* to be one second. Let us suppose three-fourths of the second exhausted, and the pendulum at *d′*, fig. 192, in its excursion towards *b*; let the rectangular impulse then be imparted to it, sufficient to carry it to *c* in one-fourth of a second. Now the long pendulum requires that it should move from *d′* to *b* in one-fourth of a second; both impulses are therefore satisfied by the pendulum taking up the position *c′* at the end of a quarter of a second. To reach this position it must describe the curve *d′ c′*. It will manifestly return along the same curve, and at the end of another quarter of a second find itself again at *d′*. From *d′* to *d* the long pendulum requires a quarter of a second. But at the end of this time the short pendulum must be at the lower limit of its swing: both requirements are satisfied by the pendulum being at *e*. We thus obtain one arm, *c′ e*, of a curve which repeats itself to the left of *e*; so that the entire curve, due to the combination of the two vibrations, is that represented in fig. 180. This figure is a parabola, whereas the figure of 8 before obtained is a lemniscata.

Fig. 192.



We have here supposed that at the moment when the rectangular impulse was applied, the motion of the pendulum was *towards b*: if it were towards *a*, we should obtain the inverted parabola, as shown in fig. 193.

Fig. 193.



Supposing, finally, the impulse to be applied, not when the pendulum is passing through its position of equilibrium, nor when it is passing a point corresponding to three-fourths or one-fourth of the time of its excursion, but at some other point in the line, *a b*, between its end and centre. Under these circumstances we should have neither the parabola nor the perfectly symmetrical figure of 8, but a distorted 8.

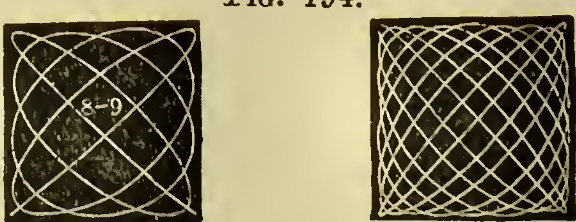
And now we are prepared to witness with profit the combined vibration of our two tuning-forks, one of which sounds the octave of the other. Permitting the vertical fork, T, fig. 187, to remain undisturbed in front of the lamp, we can oppose to it a horizontal fork, which vibrates with twice the rapidity. The first passage of the bow across the two forks reveals the exact similarity of this combination and that of our pendulum. A very perfect figure of 8 is described upon the screen. Before the lecture the vibrations of these two forks were fixed as nearly as possible to the ratio of 1 : 2, and the steadiness of the figure indicates the perfection of the tuning. Stopping both forks, and again agitating them, we have the distorted 8 upon the screen. A few trials enable me to bring out the parabola. In all these cases the figure remains fixed upon the screen. But if a morsel of wax be attached to one of the forks, the figure is steady no longer, but passes from the perfect 8 into the distorted one, thence into the parabola, from which it afterwards opens out to an 8 once more. By augmenting the discord, we can render those changes as rapid as we please.

Our next combination will be that of two forks vibrating in the ratio of 2 : 3. Observe the admirable steadiness of the figure produced by the compounding of these two rates of vibration. On attaching a fourpenny-piece with wax to one of the forks the steadiness ceases, and we have an apparent rocking to and fro of the luminous figure. Passing on to intervals of 3 : 4, 4 : 5, and 5 : 6, the figures become more intricate as we proceed. The last combination, 5 : 6, is so entangled, that to see the figure plainly a very narrow band of light must be employed. The distance existing between the forks and the screen also helps us to unravel the complication.

When the figure is fully developed, the loops along the vertical and horizontal edges express the ratio of the combined vibrations. In the octave, for example, we have two loops in one direction, and one in another; in the fifth, two loops in one direction, and three in another. When the combination is as 1 : 3, the luminous loops are also as 1 : 3. The changes which some of these figures undergo, when the tuning is not perfect, are extremely remarkable. In the case of 1 : 3, for example, it is difficult at times not to believe that you are looking at a solid link of white-hot metal. The figure exhibits a depth, apparently incompatible with its being traced upon a plane surface.

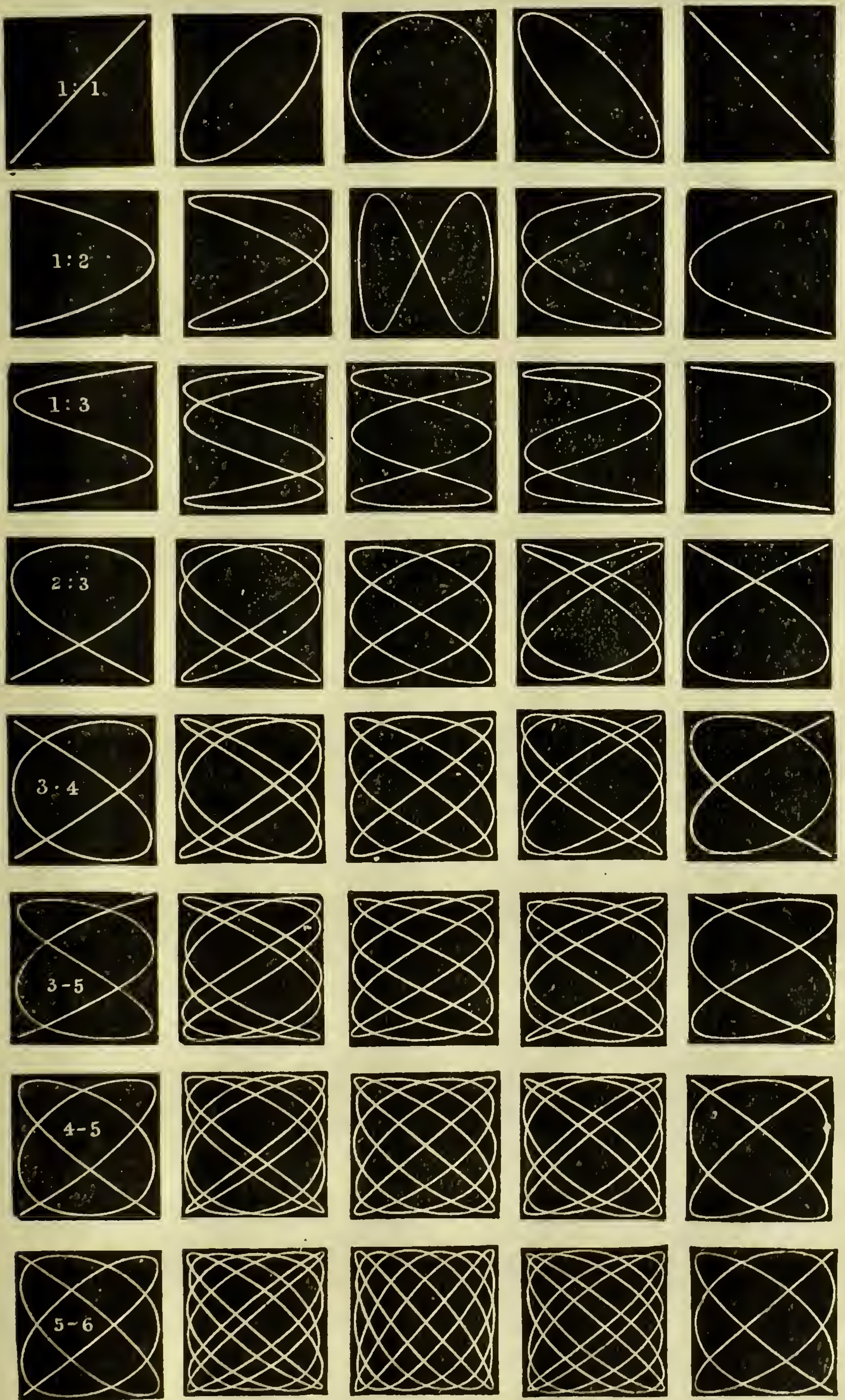
Fig. 195 is a diagram of these beautiful figures, including combinations from 1 : 1 to 5 : 6. In each case the characteristic phases of the vibration are shown; and through all of these each figure passes when the interval

Fig. 194.



between the two forks is not pure. I also add here, fig. 194, two phases of the combination 8 : 9.

FIG. 195.



To these illustrations of rectangular vibrations I add two others, figs. 196 and 197, from a very beautiful series obtained by Mr. Hubert Airy with a compound pendulum. The experiments are described in 'Nature' for August 17 and September 7, 1871. As their loops indicate, the figures are those of an octave, and a twelfth.

But the most instructive apparatus for the compounding of rectangular vibrations, is that of Mr. Tisley. Figs. 198 and 199 are copies of figures obtained by him through the joint action of two distinct pendulums; the rates of vibration corresponding to these particular figures being 2 : 3 and 3 : 4 respectively. The pen which traces the figures is moved simultaneously by two rods attached to the pendulums above their places of suspension. These two rods lie in the two planes of vibration, being at right angles to the pendulums, and to each other. At their place of intersection is the pen. By means of a ball and socket, of a special kind, the rods are enabled to move with a minimum of friction in all directions, while the rates of vibration are altered, in a moment, by the shifting of movable weights. The figures are drawn either with ink on paper, or, when projection on a screen is desired, by a sharp point on smoked glass. When the pendulums, having gone through the entire figure, return to their starting-point, they have lost a little in amplitude. The second excursion will, therefore, be smaller than the first, and the third smaller than the second. Hence the series of fine lines, enclosing gradually diminishing areas, shown in these exquisite figures.[1] Mr. Tisley's apparatus reflects the highest credit upon its able constructor.

Sir Charles Wheatstone devised, many years ago, a small and very efficient apparatus for the compounding

[1] For some beautiful figures of this description I am indebted to Professor Lynam of Yale College.

of rectangular vibrations. A drawing, fig. 200, and a description of this beautiful little instrument, for both

FIG. 196. 1 : 2.

FIG. 197. 1 : 3.

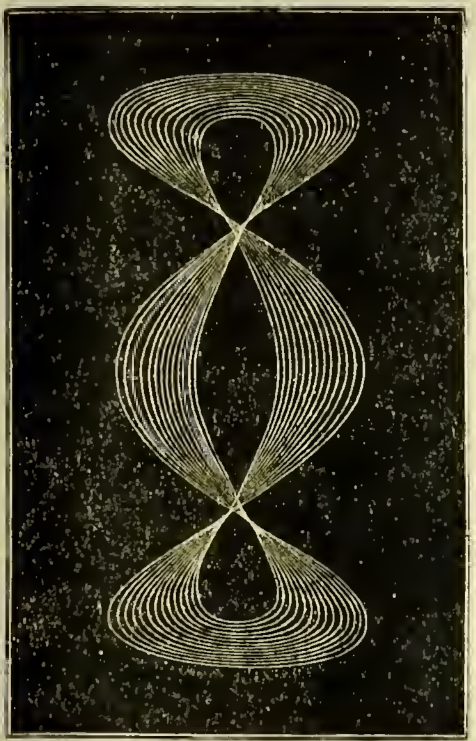


FIG. 198. 2 : 3.

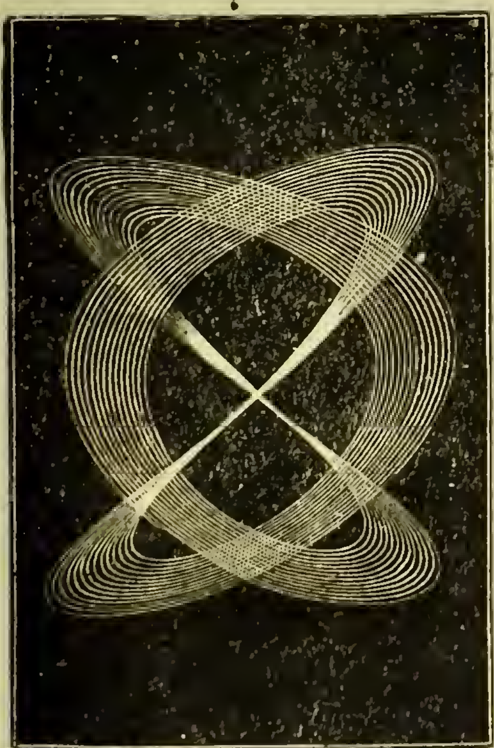
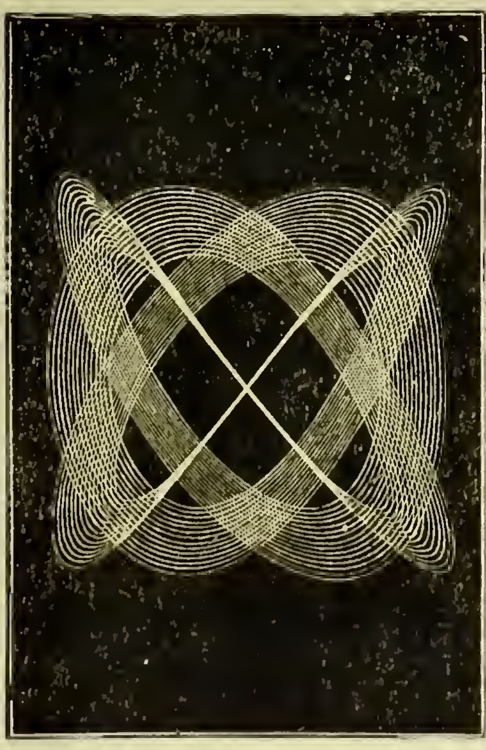


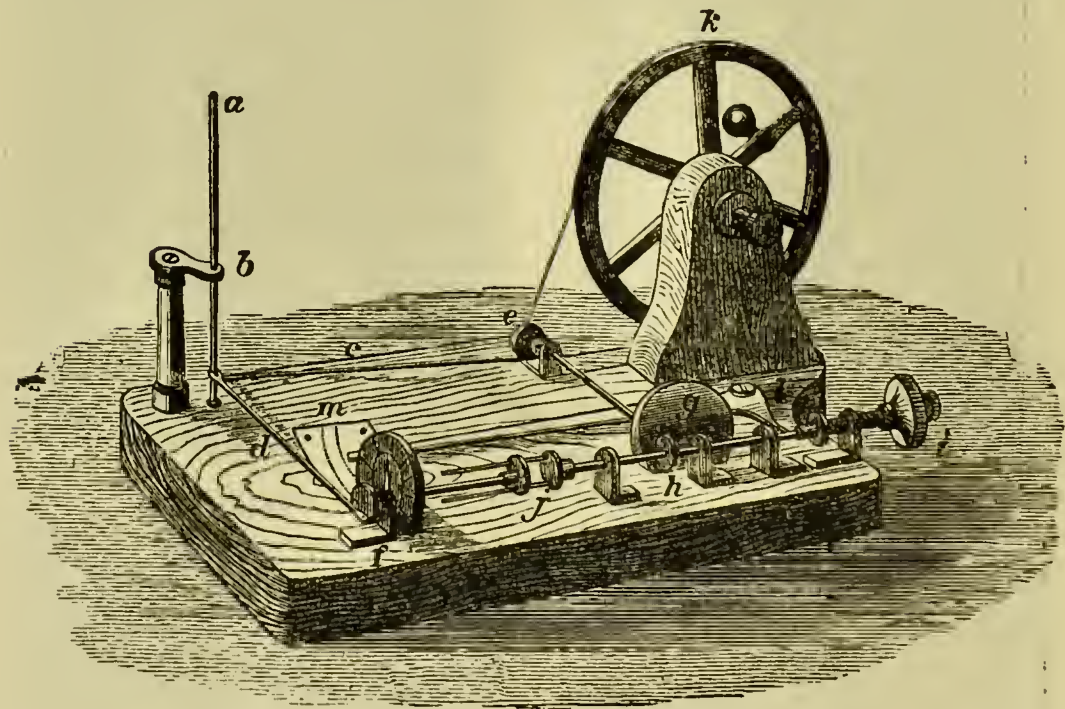
FIG. 199. 3 : 4.



which I am indebted to its eminent inventor, may find a place here. *a* is a steel rod polished at its upper end so as to reflect a point of light; this rod moves in a ball

and socket joint at *b*, so that it may assume any position. Its lower end is connected with two arms *c* and *d*, placed at right angles to each other, the other ends of which are respectively attached to the circumferences of the two circular discs *e* and *f*. The axis of the disc *e* carries at its opposite end another larger disc *g*, which gives motion

Fig. 200.



to the small disc *h*, placed on the axis which carries the disc *f*; and, according as this small disc *h* is placed nearer to or farther from the centre of the disc *g*, it communicates a different relative motion to the disc *f*. The nut and screw *i* enable the disc *h* to be placed in any position between the centre and circumference of the larger disc *g*; and by means of the fork *j* the disc *f* is caused to revolve, whatever may be the position of the disc *h*. By this arrangement, while the wheel *k* is turned regularly, the rod *a* is moved backwards and forwards by the disc *e* in one direction and by the disc *f*, with any relative oscillatory motion, in the rectangular direction. The end of the rod is thus made to describe and to exhibit optically

all the beautiful acoustical figures produced by the composition of vibrations of different periods in directions rectangular to each other. A lever *l*, bearing against the nut *i*, indicates on a scale *j* the numerical ratio of the two vibrations.[1]

I close these remarks on the combination of rectangular vibrations with a brief reference to an apparatus constructed by Mr. A. E. Donkin, of Exeter College, Oxford, and described in the 'Proceedings of the Royal Society,' vol. xxii. p. 196. In its construction great mechanical knowledge is associated with consummate skill. I saw the apparatus as a wooden model, before it quitted the hands of its inventor, and was charmed with its performance. It is now constructed by Messrs. Tisley and Spiller.

[1] Mr. Sang, of Edinburgh, was, I believe, the first to treat this subject analytically.

SUMMARY OF LECTURE IX.

By the division of a string Pythagoras determined the consonant intervals in music, proving that the simpler the ratio of the two parts into which the string was divided, the more perfect is the harmony of the sounds emitted by the two parts of the string. Subsequent investigators showed that the strings act thus because of the relation of their lengths to their rates of vibration.

With the double syren this law of consonance is readily illustrated. Here the most perfect harmony is the unison, where the vibrations are in the ratio of 1 : 1. Next comes the octave, where the vibrations are in the ratio of 1 : 2. Afterwards follow in succession the fifth, with a ratio of 2 : 3; the fourth, with a ratio of 3 : 4; the major third, with a ratio of 4 : 5; and the minor third, with a ratio of 5 : 6. The interval of a tone, represented by the ratio 8 : 9, is dissonant, while that of a semi-tone, with a ratio of 15 : 16, is a harsh and grating dissonance.

The musical interval is independent of the absolute number of the vibrations of the two notes, depending only on the *ratio* of the two rates of vibration.

The Pythagoreans referred the pleasing effect of the consonant intervals to number and harmony, and connected them with 'the music of the spheres.' Euler explained the consonant interval by reference to the constitution of the mind, which, he affirmed, took pleasure in simple calculations. The mind was fond of order, but of such order as involved no weariness in its contemplation. This peasure was afforded by the simpler ratios in the case of music.

The researches of Helmholtz prove the rapid succession of beats to be the real cause of dissonance in music.

By means of two singing flames, the pitch of one of them being changeable by the telescopic lengthening of its tube, beats of any degree of slowness or rapidity may be produced. Commencing with beats slow enough to be counted, and gradually increasing their rapidity, we reach, without breach of continuity, downright dissonance.

But to grasp this theory in all its completeness, we must refer to the constitution of the human ear. We have first the tympanic membrane, which is the anterior boundary of the drum of the ear. Across the drum stretches a series of little bones, called respectively the *hammer*, the *anvil*, and the *stirrup bone*; the latter abutting against a second membrane, which forms part of the posterior boundary of the drum. Beyond this membrane is the labyrinth filled with water, and having its lining membrane covered with the filaments of the auditory nerve.

Every shock received by the tympanic membrane is transmitted through the series of bones to the opposite membrane; thence to the water of the labyrinth, and thence to the auditory nerve.

The transmission is not direct. The vibrations are in the first place taken up by certain bodies, which can swing sympathetically with them. These bodies are of three kinds: the otolithes, which are little crystalline particles; the bristles of Max Schultze; and the fibres of Corti's organ. This latter is to all intents and purposes a stringed instrument, of extraordinary complexity and perfection, placed within the ear.

As regards our present subject, the strings of Corti's organ probably play an especially important part. That one string should respond, in some measure, to another, it

is not necessary that the unison should be perfect; a certain degree of response occurs in the immediate neighbourhood of unison.

Hence each of two strings, not far removed from each other in pitch, can cause a third string, of intermediate pitch, to respond sympathetically. And if the two strings be sounded together, the beats which they produce are propagated to the intermediate string.

So, as regards Corti's organ, when single sounds of various pitches, or rather when vibrations of various rapidities, fall upon its strings, the vibrations are responded to by the particular string whose period coincides with theirs. And when two sounds, close to each other in pitch, produce beats, the intermediate Corti's fibre is acted on by both, and responds to the beats.

In the middle and upper portions of the musical scale the beats are most grating and harsh when they succeed each other at the rate of 33 per second. When they occur at the rate of 132 per second, they cease to be sensible.

The perfect consonance of certain musical intervals is due to the absence of beats. The imperfect consonance of other intervals is due to their existence. And here the overtones play a part of the utmost importance. For though the primaries may sound together without any perceptible roughness, the overtones may be so related to each other as to produce harsh and grating beats. A strict analysis of the subject proves that intervals which require large numbers to express them are invariably accompanied by overtones which produce beats; while in intervals expressed by small numbers the beats are practically absent.

The graphic representation of the consonances and dissonances of the musical scale, by Helmholtz, furnishes a striking proof of this explanation.

The optical illustration of the musical intervals has been effected in a very beautiful manner by Lissajous. Corresponding to each interval is a definite figure, produced by the combination of its vibrations.

The compounding of vibrations has, of late years, been beautifully illustrated by apparatus constructed by Sir C. Wheatstone, Mr. Herbert Airy, and Mr. A. E. Donkin; and by the beautiful pendulum apparatus of Mr. Tisley.

APPENDICES.

APPENDIX I.

ON THE INFLUENCE OF MUSICAL SOUNDS ON THE FLAME OF A JET OF COAL-GAS. BY JOHN LECONTE, M.D.[1]

A SHORT time after reading Prof. John Tyndall's excellent article 'On the Sounds produced by the Combustion of Gases in Tubes,'[2] I happened to be one of a party of eight persons assembled after tea for the purpose of enjoying a private musical entertainment. Three instruments were employed in the performance of several of the grand trios of Beethoven, namely, the piano, violin, and violoncello. Two '*fish-tail*' gas-burners projected from the brick wall near the piano. Both of them burnt with remarkable steadiness, the windows being closed and the air of the room being very calm. Nevertheless it was evident that *one* of them was under a pressure nearly sufficient to make it *flare*.

Soon after the music commenced, I observed that the flame of the last-mentioned burner exhibited pulsations in height which were *exactly synchronous* with the audible beats. This phenomenon was very striking to everyone in the room, and especially so when the strong notes of the violoncello came in. It was exceedingly interesting to observe how perfectly even the *trills* of this instrument were reflected on the sheet of flame. A *deaf man might have seen the harmony.* As the evening advanced,

[1] This able paper was the starting-point of the experiments on sensitive flames, recorded in Lectures VI. and VII.; the researches of Thomas Young and Savart being the starting-point of the experiments on smoke-jets and water-jets.—J. T.

[2] *Phil. Mag.* S. 4, vol. xiii. p. 473, 1857.

and the diminished consumption in the city of gas *increased the pressure*, the phenomenon became more conspicuous. The *jumping* of the flame gradually increased, became somewhat irregular, and finally it began to flare continuously, emitting the characteristic sound indicating the escape of a greater amount of gas than could be properly consumed. I then ascertained by experiment, that the phenomenon *did not* take place unless the discharge of gas was so regulated that the flame approximated to the condition of *flaring.* I likewise determined by experiment, that the effects *were not* produced by jarring or shaking the floor and walls of the room by means of repeated concussions. Hence it is obvious that the pulsations of the flame *were not* owing to *indirect* vibrations propagated through the medium of the walls of the room to the burning apparatus, but must have been produced by the *direct* influence of the aerial sonorous pulses on the burning jet.

In the experiments of M. Schaffgotsch and Prof. J. Tyndall, it is evident that 'the shaking of the singing flame within the glass tube,' produced by the voice or the syren, was a phenomenon perfectly analogous to what took place under my observation *without the intervention of a tube.* In my case the discharge of gas was so regulated that there was a tendency in the flame to flare, or to emit a '*singing sound.*' Under these circumstances, strong aerial pulsations occurring at *regular intervals* were sufficient to develop synchronous fluctuations in the height of the flame. It is probable that the effects would be more striking when the tones of the musical instrument are *nearly* in unison with the sounds which would be produced by the flame, under the slight increase in the rapidity of discharge of gas, required to manifest the phenomenon of flaring. This point might be submitted to an experimental test.

As in Prof. Tyndall's experiments on the jet of gas burning within a tube, clapping of the hands, shouting, &c., were ineffectual in converting the 'silent' into the 'singing flame,' so in the case under consideration, *irregular* sounds did not produce any perceptible influence. It seems to be necessary that the impulses should *accumulate*, in order to exercise an appreciable effect.

With regard to the mode in which the sounds are produced by the combustion of gases in tubes, it is universally admitted

that the explanation given by Prof. Faraday in 1818 is essentially correct. It is well known that he referred these sounds to the successive explosions produced by the periodic combination of the atmospheric oxygen with the issuing jet of gas. While reading Prof. J. Plateau's admirable researches (third series) on the 'Theory of the Modifications experienced by Jets of Liquid issuing from Circular Orifices when exposed to the Influence of Vibratory Motions,' [1] the idea flashed across my mind, that the phenomenon which had fallen under my observation was nothing more than a *particular case* of the effects of sounds on *all kinds of fluid jets.* Subsequent reflection has only served to fortify this first impression.

The beautiful investigations of Felix Savart on the influence of sounds on jets of water, afford results presenting so many points of analogy with their effects on the jet of burning gas, that it may be well to inquire whether both of them may be referred to a common cause. In order to place this in a striking light, I shall subjoin some of the results of Savart's experiments. Vertically descending jets of water receive the following modifications under the influence of vibrations :—

1. The continuous portion becomes shortened; the vein resolves itself into separate drops nearer the orifice than when *not* under the influence of vibrations.

2. Each of the masses, as they detach themselves from the extremity of the continuous part, becomes flattened alternately in a vertical and horizontal direction, presenting to the eye, under the influence of their translatory motion, regularly disposed series of maxima and minima of thickness, or ventral segments and nodes.

3. The foregoing modifications become much more developed and regular when a note, in unison with that which would be produced by the shock of the discontinuous part of the jet against a stretched membrane, is sounded in its neighbourhood. The continuous part becomes considerably shortened, and the ventral segments are enlarged.

4. When the note of the instrument is *almost* in unison, the continuous part of the jet is alternately lengthened and shortened, and the beats which coincide with these variations in length *can be recognised by the ear.*

[1] *Phil. Mag.* S. 4, vol. xiv. p. 1, *et seq.*, July 1857.

5. Other tones act with less energy on the jet, and some produce no sensible effect.

When a jet is made to ascend *obliquely*, so that the discontinuous part appears scattered into a kind of *sheaf* in the same vertical plane, M. Savart found,—

a. That under the influence of vibrations of a determinate period, this sheaf may form itself into *two* distinct jets, each possessing regularly-disposed ventral segments and nodes; sometimes with a different node, the sheaf becomes replaced by *three* jets.

b. The note which produces the greatest shortening of the continuous part, always reduces the whole to a *single* jet, presenting a perfectly regular system of ventral segments and nodes.

In the last memoir of M. Savart—a posthumous one, presented to the Academy of Sciences of Paris, by M. Arago, in 1853,[1]—several remarkable acoustic phenomena are noticed in relation to the musical tones produced by the efflux of liquids through short tubes. When certain precautions and conditions are observed (which are minutely detailed by this able experimentalist), the discharge of the liquid gives rise to a succession of musical tones of great intensity and of a peculiar quality, somewhat analogous to that of the human voice. That these notes were not produced by the descending drops of the liquid vein, was proved by permitting it to discharge itself into a vessel of water, while the orifice was below the surface of the latter. In this case the jet of liquid must have been *continuous*, but nevertheless the notes were produced. These unexpected results have been entirely confirmed by the more recent experiments of Prof. Tyndall.[2]

According to the researches of M. Plateau, all the phenomena of the influence of vibrations on jets of liquid are referable to the conflict between the vibrations and the *forces of figure* ('*forces figuratrices*'). If the physical fact is admitted—and it seems to be indisputable—that a liquid cylinder attains a *limit of stability* when the proportion between its length and its diameter is in the ratio of 22 to 7, it is almost a *physical*

[1] *Comptes Rendus* for August 1853. Also *Phil. Mag.* S. 4, vol. vii. p. 186, 1854.

[2] *Phil. Mag.* S. 4, vol. viii. p. 74, 1854.

necessity that the jet should assume the constitution indicated by the observations of Savart. It likewise seems highly probable that a liquid jet, while in a transition stage to discontinuous drops, should be exceedingly sensitive to the influence of all kinds of vibrations. It must be confessed, however, that Plateau's beautiful and coherent theory does not appear to embrace Savart's last experiment, in which the musical tones were produced by a jet of water issuing under the surface of the same liquid. It is rather difficult to imagine what agency the 'forces of figure' could have, under such circumstances, in the production of the phenomenon. This curious experiment tends to corroborate Savart's original idea, that the vibrations which produce the sounds must take place in the glass reservoir itself, and that the cause must be inherent in the phenomenon of the flow.

To apply the principles of Plateau's theory to gaseous jets, we are compelled to abandon the idea of the *non-existence of molecular cohesion in gases.* But is there not abundant evidence to show that cohesion *does exist* among the particles of gaseous masses? Does not the deviation from rigorous accuracy, both in the law of Mariotte and of Gay-Lussac,—especially in the case of condensable gases, as shown by the admirable experiments of M. Regnault,—clearly prove that the hypothesis of the non-existence of cohesion in aeriform bodies is fallacious? Do not the expanding rings which ascend when a bubble of phosphuretted hydrogen takes fire in the air, indicate the existence of some cohesive force in gaseous product of combustion (aqueous vapour), whose outlines are marked by the opaque phosphoric acid? In short, does not the very *form* of the flame of a 'fish-tail' burner demonstrate that cohesion *must exist* among the particles of the issuing gas? It is well known that in this burner the single jet which issues is formed by the union of *two oblique jets* immediately before the gas is emitted. The result is a perpendicular *sheet of flame.* How is such a result produced by the mutual action of two jets, unless the force of cohesion is brought into play? Is it not obvious that such a fan-like flame must be produced by the same causes as those varied and beautiful forms of aqueous sheets, developed in the mutual action of jets of water, so strikingly exhibited by the experiments of Savart and of Magnus?

If it be granted that gases possess molecular cohesion, it seems to be physically certain that jets of gas must be subject to the same laws as those of liquid. Vibratory movements excited in the neighbourhood ought therefore to produce modifications in them analogous to those recorded by M. Savart in relation to jets of water. Flame or incandescent gas presents gaseous matter in a *visible* form, admirably adapted for experimental investigation; and *when produced by a jet*, should be amenable to the principles of Plateau's theory. According to this view, the pulsations or *beats* which I observed in the gas-flame when under the influence of musical sounds, are produced by the conflict between the aerial vibrations and the 'forces of figure' (as Plateau calls them) giving origin to periodical fluctuations of intensity, depending on the sonorous pulses.

If this view is correct, will it not be necessary for us to modify our ideas in relation to the agency of tubes in developing musical sounds by means of burning jets of gas? Must we not look upon all burning jets—as in the case of water-jets—as *musically inclined*; and that the use of tubes merely places them in a condition favourable for developing the tones? It is well known that burning jets frequently emit a *singing sound* when they are perfectly *free*. Are these sounds produced by successive explosions analogous to those which take place in glass tubes? It is very certain that, under the influence of molecular forces, any cause which tends to elongate the flame, without affecting the velocity of discharge, must tend to render it discontinuous, and thus bring about that mixture of gas and air which is essential to the production of the explosions. The influence of tubes, as well as aerial vibrations, in establishing the condition of things, is sufficiently obvious. Was not the 'beaded line' with its succession of 'luminous stars,' which Prof. Tyndall observed when a flame of olefiant gas, burning in a tube, was examined by means of a moving mirror, an indication that the flame became *discontinuous*, precisely as the continuous part of a jet of water becomes *shortened*, and resolved into isolated drops, under the influence of sonorous pulsations? But I forbear enlarging on this very interesting subject, inasmuch as the accomplished physicist last named has promised to examine it at a future period. In the hands of so sagacious a philosopher, we may anticipate a most searching investigation

of the phenomena in all their relations. In the meantime I wish to call the attention of men of science to the view presented in this article, in so far as it groups together several classes of phenomena under one head, and may be considered a partial generalisation.

From SILLIMAN'S *American Journal for January* 1858.

APPENDIX II.

ON ACOUSTIC REVERSIBILITY.[1]

On the 21st and 22nd of June, 1822, a Commission, appointed by the Bureau des Longitudes of France, executed a celebrated series of experiments on the velocity of sound. Two stations had been chosen, the one at Villejuif, the other at Montlhéry, both lying south of Paris, and 11·6 miles distant from each other. Prony, Mathieu, and Arago were the observers at Villejuif, while Humboldt, Bouvard, and Gay-Lussac were at Montlhéry. Guns, charged sometimes with 2 lbs. and sometimes with 3 lbs. of powder, were fired at both stations, and the velocity was deduced from the interval between the appearance of the flash and the arrival of the sound.

On this memorable occasion it was noticed that while every report of the cannon fired at Montlhéry was heard with the greatest distinctness at Villejuif, by far the greatest number of the reports from Villejuif failed to reach Montlhéry. Had wind existed, and had it blown from Montlhéry to Villejuif, it would have been recognised as the cause of the observed difference; but the air at the time was calm, the slight motion of translation actually existing being from Villejuif towards Montlhéry, or against the direction in which the sound was best heard.

So marked was the difference in transmissive power between the two directions, that on June 22, while every shot fired at Montlhéry was heard 'à merveille' at Villejuif, but one shot

[1] *Proceedings of the Royal Institution*, January 15, 1875.

out of twelve fired at Villejuif was heard, and that feebly, at the other station.

Arago in his report[1] made no attempt to explain this anomaly. His words are :—'Quant aux différences si remarquables d'intensité que le bruit du canon a toujours présentées suivant qu'il se propageait du nord au sud entre Villejuif et Montlhéry, ou du sud au nord entre cette seconde station et la première ; nous ne chercherons pas aujourd'hui à l'expliquer, parce que nous ne pourrions offrir au lecteur que des conjectures denuées de preuves.'[2]

Some years ago I tried to bring this subject within the range of experiment. The first step was to ascertain whether the sensitive flame, referred to in my recent paper in the 'Philosophical Transactions,' could be safely employed in experiments on the mutual reversibility of a course of sound and an object on which the sound impinges. Now, the sensitive flame usually employed by me measures from 18 to 24 inches in height, while the reed employed as a source of sound is less than a square quarter of an inch in area. If, therefore, the whole flame, or the pipe which fed it, were sensitive to sonorous vibrations, strict experiments on reversibility with the reed and flame might be difficult, if not impossible. Hence my desire to learn whether the seat of sensitiveness was so localised in the flame as to render the contemplated interchange of flame and reed permissible.

The flame being placed behind a cardboard screen, the shank of a funnel passed through a hole in the cardboard was directed upon the middle of the flame. The sound-waves issuing from the vibrating reed, placed within the funnel, produced no sensible effect. Shifting the funnel so as to direct its shank upon the root of the flame, the action was violent.

To augment the precision of the experiment, the funnel was connected with a glass tube 3 feet long and half an inch in diameter, the object being to weaken, by distance, the effect of the waves diffracted round the edge of the funnel, and to permit those only which passed through the glass tube to act upon the flame.

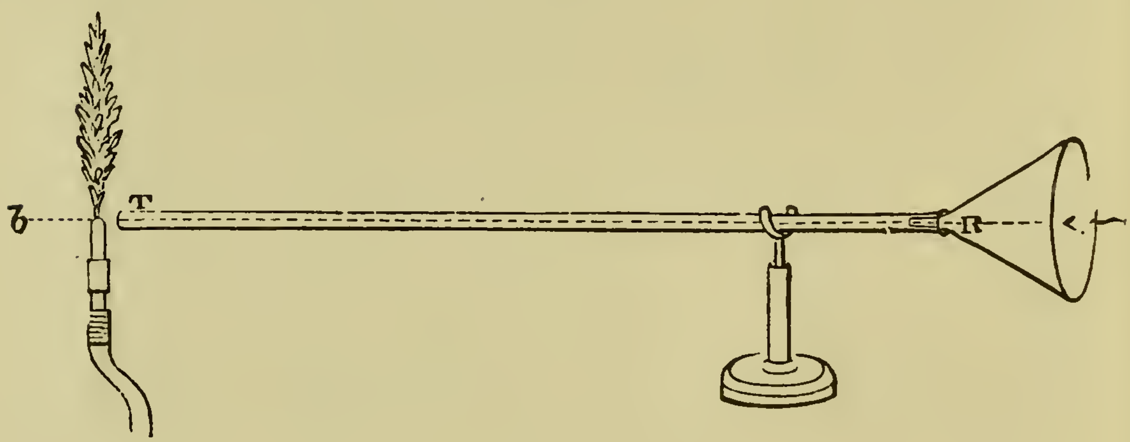
[1] *Researches in Chemistry and Physics*, p. 484.

[2] *Connaissance des Temps*, 1825, p. 370.

Presenting the end of the tube to the orifice of the burner, *b* (fig. 1), or the orifice to the end of the tube, the flame was violently agitated by the sounding-reed, R. On shifting the tube, or the burner, so as to concentrate the sound on a portion of the flame about half an inch above the orifice, the action was *nil.* Concentrating the sound upon the burner itself, about half an inch below its orifice, there was no action.

These experiments demonstrate the localisation of 'the seat of sensitiveness,' and they prove the flame to be an appropriate instrument for the contemplated experiments on reversibility.

FIG. 1.



The experiments then proceeded thus :—The sensitive flame being placed behind a screen of cardboard 18 inches high by 12 inches wide, a vibrating reed, standing at the same height as the root of the flame, was placed at a distance of 6 feet on the other side of the screen. The sound of the reed, in this position produced a strong agitation of the flame.

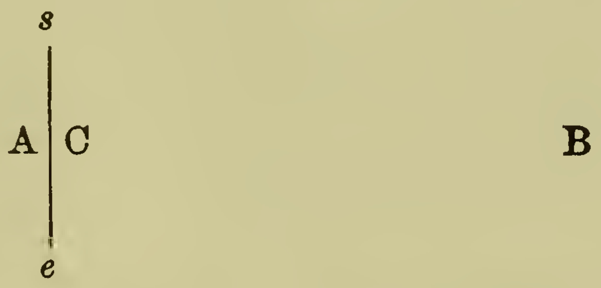
The whole upper half of the flame was here visible from the reed ; hence the necessity of the foregoing experiments to prove the action of the sound on the upper portion of the flame to be *nil*, and that the waves had really to bend round the edge of the screen, so as to reach the seat of sensitiveness in the neighbourhood of the burner.

The positions of the flame and reed were reversed, the latter being now close behind the screen, and the former at a distance of 6 feet from it. The sonorous vibrations were without sensible action upon the flame.

The experiment was repeated and varied in many ways.

Screens of various sizes were employed; and, instead of reversing the positions of the flame and reed, the screen itself was moved, so as to bring, in some experiments the flame, and in other experiments the reed, close behind it. Care was also taken that no reflected sound from the walls or ceiling of the laboratory, or from the body of the experimenter, should have anything to do with the effect. In all cases it was shown that the sound was effective when the reed was at a distance from the screen, and the flame close behind it; while the action was insensible when these positions were reversed.

Fig. 2.



Thus, let *s e* (fig. 2) be a vertical section of the screen. When the reed was at A and the flame at B there was no action; when the reed was at B and the flame at A the action was decided. It may be added that the vibrations communicated to the screen itself, and from it to the air beyond it, were without effect; for when the reed, which at B was effectual, was shifted to C, where its action on the screen was greatly augmented, it ceased to have any action on the flame at A.

We are now, I think, prepared to consider the failure of reversibility in the larger experiments of 1822. Happily an incidental observation of great significance comes here to our aid. It was observed and recorded at the time that, while the reports of the guns at Villejuif were without echoes, a roll of echoes, lasting from 20 to 25 seconds, accompanied every shot at Montlhéry, being heard by the observers there. Arago, the writer of the report, referred these echoes to reflection from the clouds—an explanation which I think we are now entitled to regard as problematical. The report says that 'tous les coups tirés à Montlhéry y étaient *accompagnés* d'un roulement semblable à celui du tonnerre.' I have italicised a very significant word—a word which fairly applies to our experiments on gun sounds at the South Foreland, where there was no sensible

interval between explosion and echo, but which could hardly apply to echoes coming from the clouds. For, supposing the clouds to be only a mile distant, the sound and its echo would have been separated by an interval of nearly ten seconds. But there is no mention of any interval; and had such existed, surely the word 'followed,' instead of 'accompanied,' would have been the one employed. The echoes, moreover, appear to have been *continuous*, while the clouds observed seem to have been *separate*. 'Ces phénomènes,' says Arago, 'n'ont jamais eu lieu qu'au moment de l'apparition de quelques nuages.' But from separate clouds a continuous roll of echoes could hardly come. When to this is added the experimental fact that clouds far denser than any ever formed in the atmosphere are demonstrably incapable of sensibly reflecting sound, while cloudless air, which Arago pronounced echo-less, has been proved capable of powerfully reflecting it, I think we have strong reason to question the hypothesis of the illustrious French philosopher.

And, considering the hundreds of shots fired at the South Foreland, with the attention especially directed to the aerial echoes, when no single case occurred in which echoes of measurable duration did not accompany the report of the gun, I think Arago's statement, that at Villejuif no echoes were heard when the sky was clear must simply mean that they vanished with great rapidity. Unless the attention were specially directed to the point, a slight prolongation of the cannon-sound might well escape observation; and it would be all the more likely to do so if the echoes were so loud and prompt as to form apparently part and parcel of the direct sound.

I should be very loth to transgress here the limits of fair criticism, or to throw doubt, without good reason, on the recorded observations of illustrious men. Still, taking into account what has just been stated, and remembering that the minds of Arago and his colleagues were occupied by a totally different problem—that the echoes were an incident rather than an object of observation—I think we may justly consider the sound which he called 'instantaneous' as one whose aerial echoes did not differentiate themselves from the direct sound by any noticeable fall of intensity, and which rapidly died into silence.

Turning now to the observations at Montlhéry, we are struck by the extraordinary duration of the echoes heard at that station. At the South Foreland the charge habitually fired was equal to the largest of those employed by the French philosophers; but on no occasion did the gun-sound produce echoes approaching to 20 or 25 seconds duration. The time rarely reached half this amount. Even the syren-echoes, which were more remarkable and more long-continued than those of the gun, never reached the duration of the Montlhéry echoes. The nearest approach to it was on October 17, 1873, when the syren-echoes required 15 seconds to subside into silence.

On this same day, moreover (and this is a point of marked significance), the transmitted sound reached its maximum range, the gun-sounds being heard at the Quenocs buoy, 16½ nautical miles from the South Foreland. I have stated in another place that the duration of the air-echoes indicates 'the atmospheric depths' from which they come.[1] An optical analogy may help us here. Let light fall upon solid chalk, the light is wholly scattered by the superficial particles; let the chalk be powdered and mixed with water, light reaches the observer from a far greater depth of the turbid liquid. The solid chalk typifies the action of exceedingly dense acoustic clouds; the chalk and water that of clouds of more moderate density. In the one case we have echoes of short, in the other echoes of long, duration. These considerations prepare us for the inference that Montlhéry, on the occasion referred to, must have been surrounded by a highly diacoustic atmosphere; while the shortness of the echoes at Villejuif shows that the atmosphere surrounding that station must have been, in a high degree, acoustically opaque.

Have we any clue to the cause of the opacity? I think we have. Villejuif is close to Paris, and over it, with the observed light wind, was slowly wafted the air from the city. Thousands of chimneys to windward of Villejuif were discharging their heated currents; so that an exceedingly non-homogeneous atmosphere probably surrounded that station.[2] At no great height in the atmosphere the equilibrium of temperature would

[1] See page 313.

[2] The effect of the air of London is sometimes strikingly evident.

be established. This non-homogeneous air surrounding Villejuif is experimentally typified by our screen, with the source of sound close behind it, the upper end of the screen representing the place where equilibrium of temperature was established in the atmosphere above the station. In virtue of its proximity to the screen, the echoes from our sounding-reed would, in the case here supposed, so blend with the direct sound as to be practically indistinguishable from it, as the echoes at Villejuif followed the direct sound so hotly, and vanished so rapidly, that they escaped observation. And as our sensitive flame, at a distance, failed to be affected by the sounding body placed close behind the cardboard screen, so, I take it, did the observers at Montlhéry fail to hear the sounds of the Villejuif gun.

28

Something further may be done towards the experimental elucidation of this subject. The facility with which sounds pass through textile fabrics has been already illustrated,[1] a layer of cambric or calico, or even of thick flannel or baize, being found competent to intercept but a small portion of the sound from a vibrating reed. Such a layer of calico may be taken to represent a layer of air, differentiated from the air adjacent by temperature or moisture; while a succession of such sheets of calico may be taken to represent successive layers in non-homogeneous air.

Two tin tubes, M N and O P (fig. 3), with open ends were

FIG. 3.

placed so as to form an acute angle with each other. At the end of one was the vibrating reed r; opposite the end of the other, and in the prolongation of P O, the sensitive flame f, a

[1] *Phil. Trans.* 1874, pt. i. p. 208, and Lecture VII. of this volume. p28

329

second sensitive flame (f') being placed in the continuation of the axis of M N. On sounding the reed, the direct sound through M N agitated the flame f'. Introducing the square of calico *ab* at the proper angle, a slight decrease of the action on f' was noticed, and the feeble echo from *ab* produced a barely perceptible agitation of the flame *f*. Adding another square, *cd*, the sound transmitted by *ab* impinged on *cd*; it was partially echoed, returned through *ab*, passed along P O, and still further agitated the flame *f*. Adding a third square, *ef*, the reflected sound was still further augmented, every accession to the echo being accompanied by a corresponding withdrawal of the vibrations from f', and a consequent stilling of that flame.

With thinner calico or cambric it would require a greater number of layers to intercept the entire sound; hence with such cambric we should have echoes returned from a greater distance, and therefore of greater duration. Eight layers of the calico employed in these experiments, stretched on a wire frame and placed close together as a kind of pad, may be taken to represent a dense acoustic cloud. Such a pad, placed at the proper angle beyond N, cuts off the sound, which in its absence reaches f', to such an extent that the flame f', when not too sensitive, is thereby stilled, while *f* is far more powerfully agitated than by the reflection from a single layer. With the source of sound close at hand, the echoes from such a pad would be of insensible duration. Thus close at hand do I suppose the acoustic clouds surrounding Villejuif to have been, a similar shortness of echo being the consequence.

A further step is here taken in the illustration of the analogy between light and sound. Our pad acts chiefly by internal reflection. The sound from the reed is a composite one, made up of partial sounds differing in pitch. If these sounds be ejected from the pad in their pristine proportions, the pad is acoustically *white*; if they return with their proportions altered, the pad is acoustically *coloured*.

In these experiments my assistant, Mr. Cottrell, has rendered me material assistance.[1]

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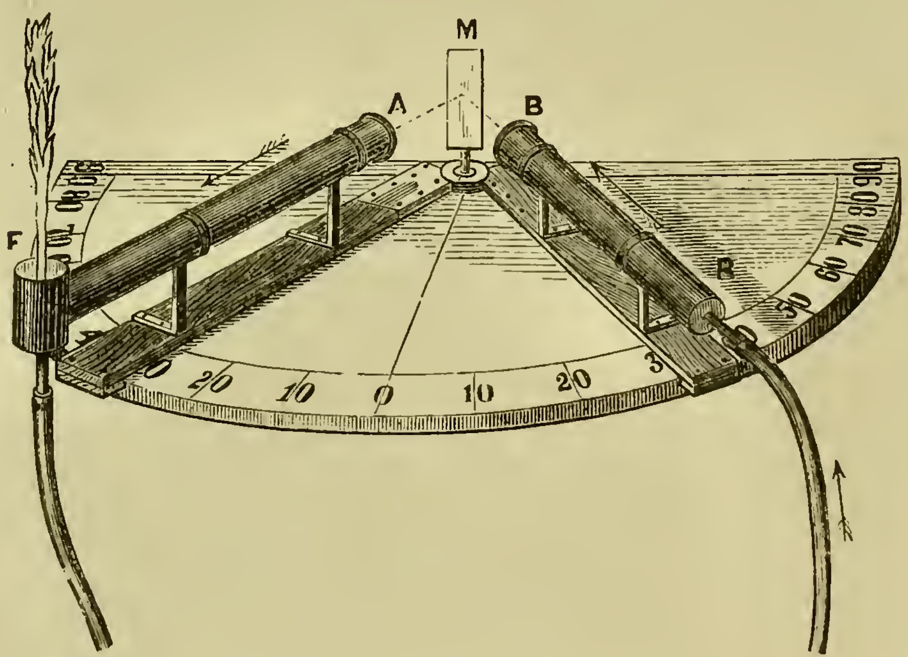
[1] Since this was written I have sent the sound through fifteen layers of calico, and echoed it back through the same layers, in strength sufficient to agitate the flame. Thirty layers were here crossed by the sound. The

NOTE, *June* 3.—I annex here a sketch of an apparatus devised by my assistant and constructed by Tisley and Spiller, for the demonstration of the law of reflection of sound. It consists of two tubes (AF, BR), with a source of sound at the end R of one of them, and a sensitive flame at the end F of the other. The axes of the tubes converge upon a mirror M, and they are capable of being placed so as to enclose any required angle. The angles of incidence and reflection are read off on the graduated semicircle. The mirror M is also movable round a ver ical axis.



Remarkable Instances of Acoustic Opacity.

For the following account of the battle of Gain's Farm, I am indebted to the Rector of the University of Virginia :—

'Lynchburgh, Virginia :
'March 19, 1874.

'SIR,—I have just read with great interest your lecture of January 16, on the acoustic transparency and opacity of the

sound was subsequently found able to penetrate two hundred layers of cotton net; a single layer of wetted calico being competent to stop it.

Lord Rayleigh, for whose judgment I have the greatest respect, has questioned the foregoing explanation of the phenomena recorded by Arago. Whatever becomes of the explanation, the facts above recorded will stand, and for their sake I retain the paper.

atmosphere. The remarkable observations you mention induce me to state to you a fact which I have occasionally mentioned, but always, where I am not well known, with the apprehension that my veracity would be questioned. It made a strong impression on me at the time, but was an insoluble mystery until your discourse gave me a possible solution.

'On the afternoon of June 28, 1862, I rode, in company with General G. W. Randolph, then Secretary of War of the Confederate States, to Price's house, about nine miles from Richmond. The evening before, General Lee had begun his attack on M'Clellan's army, by crossing the Chickahominy about four miles above Price's, and driving in M'Clellan's right wing. The battle of Gain's Farm was fought the afternoon to which I refer. The valley of Chickahominy is about one and a half mile wide from hill-top to hill-top. Price's is on one hill-top, that nearest to Richmond; Gain's Farm, just opposite, is on the other, reaching back in a plateau to Cold Harbour.

'Looking across the valley I saw a good deal of the battle, Lee's right resting in the valley, the Federal left wing the same. My line of vision was nearly in the line of the lines of battle. I saw the advance of the Confederates, their repulse two or three times, and in the grey of the evening the final retreat of the Federal forces.

'I distinctly saw the musket fire of both lines, the smoke, individual discharges, the flash of the guns. I saw batteries of artillery on both sides come into action and fire rapidly. Several field-batteries on each side were plainly in sight. Many more were hid by the timber which bounded the range of vision.

'Yet looking for nearly two hours, from about 5 to 7 P.M. on a midsummer afternoon, at a battle in which at least 50,000 men were actually engaged, and doubtless at least 100 pieces of field-artillery, through an atmosphere optically as limpid as possible, *not a single sound of the battle* was audible to General Randolph and myself. I remarked it to him at the time as astonishing.

'Between me and the battle was the deep broad valley of the Chickahominy, partly a swamp shaded from the declining sun by the hills and forest in the west (my side). Part of the valley on each side of the swamp was cleared; some in cultiva-

tion, some not. Here were conditions capable of providing several belts of air, varying in the amount of watery vapour (and probably in temperature), arranged like laminæ at right angles to the acoustic waves as they came from the battle-field to me.

'Respectfully,

'Your obedient servant,

'R. G. H. KEAN.

'Professor John Tyndall.'

I extract the following from Mr. Archibald Forbes's 'Experiences of the War between France and Germany':—

'Before ten o'clock on the morning of the 5th, the air, thick with fog, was burdened also with a sound to which one could not well give a name. . . . The morning of the 6th presented a remarkable contrast in every respect to that of the preceding day. The latter had been cold to chilling of the marrow of the bones, and so thick that nothing was to be seen half a mile away. The former was clear, bright, and warm as a morning at the end of March. Yesterday the air was charged with sound; to-day there reigned the stillness of an Arcadia that knows no war. Men looked at each other in blank amazement. Had Paris, forts, big guns, bombardment, and the non-bombardment on the eastern side alike been spirited away? . . . Determining to anticipate by personal investigation information which was kindly promised me, I rode off to the front of Montmorency, whence there lay spread before the eye the wide panorama of the north side of Paris. Still, all was silent as the grave. . . . There I found three mounted men, and we had a little talk about the position. They inclined to the armistice negotiations theory, as they had not heard a single shot since morning. As we spoke, there came a white jet of smoke out of the grey side of La Briche. No sound—for all the noise it made, it might have been an escape of steam. . . . I came on to Gonesse alone. What was my surprise to find all the German batteries from Gonesse to Sevran firing away vigorously! They had been at it since eight in the morning. In Gonesse I learned that the firing on the south side was believed to have re-commenced at the same hour, and was certainly going on. Yet at Margencey and Montmorency we could not hear a

sound. It was all owing to the air; it was to-day as non-conducting of sound as it had been the reverse yesterday. Even in Gonesse we could not hear the guns that were thundering, so to speak, at our elbows.'

The Duke of Argyll has favoured me with the following very interesting account of his experience as to the penetrability of fog by sound. 'This fact,' says his Grace, 'I have long known from having lived a great part of my life within four miles of the town of Greenock, across the Frith. Shipbuilding goes on there to a great extent, and the hammering of the caulkers and builders is a sound which I have been in the habit of hearing with every variety of distinctness, or of not hearing at all, according to the state of the atmosphere; and I have always observed on days when the air was very clear, and every mast and spar was distinctly seen, hardly any sound was heard; whereas on thick and foggy days, sometimes so thick that nothing could be seen, every clink of every hammer was audible, and appeared sometimes as close at hand.'

The Rev. George H. Hetling, of Fulham, has also written to me, with a circumstantiality which leaves no room for doubt, that he has heard the Portland guns at a distance of 44 miles through a dense fog.

Barometer

INDEX.

Items otherwise indicated are crossed out for complexi-city & for extensive reasons.

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